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AMERICAN PUBLIC HEALTH ASSOCIATION.

FIFTEENTH ANNUAL MEETING.

THE ANNUAL ADDRESS.

Delivered Tuesday, November 8, 1887.

BY GEORGE M. STERNBERG, M.D.,

PRESIDENT.

(Concluded from page 561)

As I have already intimated, the exotic pestilential diseases to which I have referred are the levers which move corporations to make necessary sanitary improvements. But for sanitarians, aside from their effect in this way, they are of secondary importance. The number of victims which they claim is a small matter compared with the number who succumb to certain indigenous or naturalized infectious diseases which are equally subject to control by well-known sanitary measures. The chief aim of the American Public Health Association should be to ascertain what measures are most effectual for the restriction of their endemic maladies, such as typhoid fever and the malarial fevers, and for the banishment of all diseases in which the contagion is given off from the persons of the sick, such as scarlet fever and smallpox. So far as diseases of the class last mentioned are concerned, we may safely say that we know how they may be banished from a community, viz., by isolation of the sick and disinfection of all infectious material; and in the case of smallpox, by vaccination. Our main mission, therefore, is to insist upon the thorough execution of these measures.

But our mission does not cease here. We have not only to teach the American public how to guard against infectious diseases by quarantine restrictions, isolation of the sick, disinfection, and municipal sanitation, but also to teach them the principles of personal hygiene. Not only will their individual susceptibility in the presence of an epidemic depend largely upon their personal habits and mode of life, but we must show them how often organic and functional diseases of the various organs essential to life are induced by excesses in diet, improper food, the habitual use of spirituous liquors, etc. I conceive that a most important part of our work in the future should consist in popularizing information of this kind. The noble example set by our generous colleague, Mr. Lomb, of Rochester, should be followed by our Association, if its finances permit, or at least we should present the subject to philanthropists who, like Mr. Lomb, may desire to be guided by our counsel in their efforts to do good to their fellowmen. As a matter of fact, our limited means will not justify us in offering prizes for essays on sanitary subjects, and the most that we can afford is to print and distribute at cost such essays as may seem suitable for popular distribution.

In another direction, however, we may accomplish much good with comparatively small amounts of money, whenever our treasury will admit of it. I refer to special investigations in sanitary science, committed to expert investigators who are willing to devote their time to the work, and who would ask only for such sums as might be necessary to cover the actual expenditures for apparatus, material, etc.

The exact knowledge which has been obtained during the last decade with reference to the etiology of the infectious diseases, has been promptly applied in a practical way by sanitarians; and every addition to our knowledge in this direction is of the greatest importance to sanitary science, which, so far as we can see, will reap far more benefit from an exact knowledge with reference to the essential characters of each specific disease germ than can be hoped for by the clinician.

It is not creditable to us as a nation that so small a share of the progress in this direction is due to American investigators. In the absence of any department of the National Government having the power and disposition to support investigations in this field of science, the few who have pursued bacteriological studies in this country have worked under disadvantages which were not only discouraging, but absolutely incompatible with the most efficient work and fruitful results. But the future is more hopeful. Individual munificence, in several of our large cities, has supplied the means which should have been provided long since by the National Government, for pursuing investigations in bacteriology and in experimental pathology.

In Baltimore we have a well-equipped bacteriological laboratory under the direction of the able professor of pathology of the Johns Hopkins University. In New York, Boston, Philadelphia, and, I believe, in several other cities, bacteriological laboratories have been established in connection with well-known medical schools.

In Brooklyn, the Hoagland Laboratory, now in process of construction, under the immediate supervision of the gentleman who provides the funds required for the building and its equipment, will supply all of the facilities, both for students and for advanced investigators, which can be found in the best equipped laboratories of Europe. I am informed, also, that our colleagues from the State of Michigan who are always in the front rank in urging upon their legislature measures which will advance the sanitary interests of the State, are making an effort, which is likely to be successful, to establish a bacteriological laboratory in that State. We have to thank one of our colleagues from the State mentioned for a most important discovery in the field of sanitary science. I refer to the discovery of tyrotoxicon by Professor Vaughan. With a well-equipped laboratory under his direction there is good reason to

believe that this discovery, so creditable to American science, would not stand alone to the credit of the good State of Michigan.

Our Association has already taken the initiative in encouraging, and assisting, from its slender treasury, investigations in sanitary science. The Committee on disinfectants, appointed at the St. Louis meeting, in 1884, has made an extended experimental research with reference to the value of various agents for the destruction of disease germs. The final report of this Committee will be submitted at the present meeting. I say final, because in my judgment all the necessary data are now at hand for determining the agents most useful for disinfecting purposes under various circumstances, and I think the conclusions of the Committee may be accepted as a safe guide for future practice. It is true that more work can be done to advantage in this direction, and it will be desirable to test from time to time new agents which may be suggested for disinfecting purposes; but the main object of the Association has been accomplished, and we now know what agents can be relied upon for the destruction of disease germs of various kinds, and in what proportion these agents must be used to be efficient.

In consideration of the limited means at our disposal for encouraging the scientific investigation of sanitary problems I would suggest that the Association raise a special fund for this purpose, by calling for contributions from its members, and from others who may be willing to aid us in our efforts in this direction. If you approve of this suggestion, I trust that some member will introduce the necessary resolutions.

One of the subjects which might be taken up by a Committee appointed for the purpose and aided from the special fund would be a biological investigation of the water supply of towns and cities in the United States. Evidently such an investigation would be a protracted one, and it would be advisable to begin with those towns which have a notoriously bad water supply. Perhaps it could be arranged that a portion of the expense, at least, would be paid by the town or city whose water supply was examined, as the inhabitants of such town or city would be especially interested in the results of the investigation. Another question of the greatest interest to us is that which relates to the possibility of protecting individuals from fatal infectious diseases by inoculations with an attenuated virus.

Protection from smallpox is no longer a solitary instance of prophylaxis by inoculation. In anthrax, in swine-plague, roguet, and in pleuro-pneumonia, protective inoculations have been practised upon a large scale, and the value of the method is fully described. The evidence in favor of Pasteur's inoculations for the prevention of hydrophobia is such that we can scarcely doubt that it has a relative value, notwithstanding the considerable number of deaths which have occurred among those who have been inoculated. The recent report of the English Commission, made after a thorough investigation, is favorable to the method, which may, perhaps, hereafter, be modified so as to give still better results.

In the various infectious diseases of the lower animals which have been studied during recent years, including those forms of septicæmia which are only known to us by laboratory experiments, we have much evidence that protective inoculations with an attenuated virus may be successfully practised. And there is good reason to hope that in all diseases in which a single attack protects from future attacks, protective inoculations may be practised when once we have succeeded in isolating and cultivating outside of the body the specific infectious agent. We know already four methods by which the virulent potency of disease germs may be attenuated, viz., by exposure to oxygen, by exposure to heat, by exposure to the action of certain chemical agents, and by passing through the body of certain animals.

The last mentioned method is that practised by Pasteur in attenuating the virus of rouget. He finds that the virulence of the microbe is diminished by passing it through a series of rabbits and that by this means an attenuated virus suitable for protective inoculations in swine may be obtained.

The possibility of attenuating a virus by exposure to the action of certain chemical agents was discovered by myself, in 1881, in the course of a series of experiments upon disinfectants in which the virus used was the blood of a rabbit just dead and containing the micrococcus found in the buccal secretions of man, which I have named *M. Pasteuri.*

Among the diseases in which there is good reason to hope that a method of prophylaxis by inoculations with an attenuated virus might be successfully practised, if we could once succeed in isolating and cultivating the specific germ of the disease, is yellow fever, a disease in which, as a rule, a single attack protects.

As you know, it has been claimed by both Dr. Domingos Freire, of Brazil, and by Dr. Carmona y Valle, of Mexico, that the yellow fever germ is discovered, and that a method of prophylaxis by inoculation has been experimentally demonstrated. As I have just returned from a special mission, the object of which was to investigate the claims of these gentlemen, you will naturally expect to hear from me at this time, with reference to the results of my investigations. I regret to say that I am unable to gratify you in this natural expectation. My orders explicitly direct "me not to make publication of my investigations and of the conclusions reached by me," until I have submitted my final report to the President of the United States.

I have already occupied so much time in speaking of matters which I consider of prime importance that I cannot attempt to make any general review of recent bacteriological researches which have a bearing upon public hygiene. As a matter of fact, all additions to our knowledge of pathogenic microörganisms are of special importance to us as sanitarians, and it is now generally recognized that the only safe basis for practical sanitation is that which is afforded by an exact knowledge of the etiology of infectious diseases, and of the biological characters of the specific infectious agents in these diseases. Since the importance of these studies has been generally recognized, and the methods of research have been perfected, the number of trained workers has rapidly increased, and at present the greatest activity prevails in the laboratories of Europe. This is shown by the number of memoirs relating to experimental investigations made in this department of science which are constantly appearing in the journals devoted to medicine and hygiene in all parts of the

world, but especially in Germany, in France, and in Italy. The *Jahresbericht* of Baumgarten, for the year 1886, contains abstracts of 533 papers relating to micro-organisms. With reference to *cholera*, I may say to you that recent researches give support to the conclusions of Koch as to the pathogenic *rôle* in this disease of the spirillum discovered by him in the intestines of cholera patients. Its constant presence in this disease seems to be demonstrated, and it is now generally admitted by bacteriologists that there are definite characters by which it may be distinguished from similar organisms obtained from other sources, such as the Finkler-Prior spirillum, and the cheese spirillum of Deneke, which closely resemble it.

Lustig, Director of the Cholera Hospitals at Trieste, examined the dejecta in 170 cases of cholera, and found the spirillum of Koch in every case: on the other hand, the bacillus of Emmerich was found in only 40 out of the whole number of cases examined. Tizzoni and Cattani also found Koch's spirillum in the contents of the intestine in 24 cases examined by them during the epidemic at Bologna in 1886. At Padua, also, researches made by Canestrini and Morpurgo gave the same result, the spirillum was constantly found in the dejecta in recent cases. These observers state that the cholera spirillum retains its motility and reproductive power for a considerable time in sterilized distilled water. They were able to obtain cultures after two months from such water. This important fact has been verified by Pfeiffer, who found, however, that in the presence of common saprophytic bacteria the cholera microbe soon died out. Hueppe has shown that the cholera spirillum forms reproductive elements, which he calls arthrospores. These are not so readily destroyed by desiccation as are the fresh bacilli, but they have nothing like the resisting power to heat and chemical agents which characterizes the endogenous spores of the bacilli. The exact proportion in which various disinfecting agents are destructive of the vitality of the cholera spirilla, has now been determined with great precision, and will be stated in detail in the report of the Committee on Disinfectants for the present year. This Committee has also made extended experiments of the same kind in which the typhoid bacillus, and various other pathogenic organisms have served as the test of germicide power. The chemical products developed in cultures as a result of the vital activity of the cholera spirillum have been studied by Bitter, Buchner, and Cantani. The last-named author claims to have demonstrated the presence of a poisonous ptomaine in cholera cultures, which, when injected into the peritoneal cavity of dogs gives rise to symptoms resembling those of cholera. A recent observation of value is that of Bujwid, who finds that *bouillon* cultures of the cholera spirillum have a peculiar chemical reaction, by which they may be distinguished. According to this author, the addition of a five to ten per cent. solution of hydrochloric acid to such a culture gives rise, within a few minutes, to a rose-violet color, which subsequently, when exposed to light, changes to a brownish shade. The reaction does not occur in impure cultures. The Finkler-Prior spirillum is said to give a similar reaction after a longer time, but the color first developed is of a more brownish hue.

The question of the etiological *rôle* and biological characters of the typhoid bacillus discovered by Eberth in 1880, has occupied numerous bacteriologists during the past year, and very important additions have been made to our knowledge with reference to this organism. The researches of Beumer and Pieper, of Seitz, and of Fränkel and Simonds, are especially worthy of notice, but time will not permit me to give an abstract of the results reached by these and other investigators. I can only say, in a general way, that the earlier researches of Eberth, Koch, and Gaffky, are confirmed as regards the presence of this bacillus in the intestinal glands, the spleen, and other organs in typhoid cases, and that very little doubt exists among bacteriologists as to the etiological relation of this organism to the disease in question, although no satisfactory proof by inoculation in lower animals has yet been found. This, however, is not surprising, inasmuch as we have no evidence that any of the animals experimented upon are liable to contract the disease, as man does, by drinking contaminated water.

According to Wolffhügel and Riedel, the typhoid bacillus and various other pathogenic organisms tested, retain their vitality for a long time when preserved in ordinary well or hydrant water, and even undergo a considerable development in such water. Frankland, also, has found that certain pathogenic bacteria tested by him increased rapidly in numbers in the water of the Thames, and even in distilled water. Meade Bolton, on the other hand, found that micrococcus tetragonus, staphylococcus aureus, the typhoid bacillus, and the anthrax bacillus, not only did not increase in number in sterilized water, but soon perished, while certain non-pathogenic species commonly found in water, increased rapidly in numbers, in sterilized distilled water. A more recent research is that of Kraus, who employed well water and hydrant water from the city water works of Munich, which, without being sterilized, was infected with pure cultures of various pathogenic organisms diluted with distilled water. The infected water was kept during the experiment at a temperature of 10½° C. (51° F.). Plate cultures were made from day to day. The results were as follows: the typhoid bacillus had disappeared by the seventh day, the cholera spirillum could not be found in plate cultures after the second day, the anthrax bacillus had disappeared at the end of four days; in the meantime, the ordinary water organisms had increased enormously in number. From these experiments, considered in connection with those of Bolton and of Wolffhügel, Kraus concludes that the rapid destruction of pathogenic bacteria in non-sterilized water is a direct result of the action of ordinary water-organisms. If this be true, it is evident that these water-bacteria are conservative from a sanitary point of view, and that the biological test of drinking water, which gives the number of colonies which are obtained from a given quantity, has no special value in the absence of an exact statement of the kind of bacteria and their pathogenic potency. The time has come when we must demand that those who undertake the biological examination of water with reference to its potability shall give some more definite information than that a certain number of colonies were found, some of which liquified gelatine, and some did not. Up to the present time we have but few instances of the finding of known pathogenic bacteria

in water used for drinking purposes. Koch found his spirillum in a water-tank in India, and several observers have reported the finding of the typhoid bacillus in drinking water. Recently Beumer examined the water of four wells in a vicinity where cases of typhoid fever had repeatedly occurred. From one of those wells colonies were obtained by the plate method which proved to have all of the characters of the typhoid bacillus. The distinguished German chemist Brieger has succeeded in obtaining a toxic ptomaine from cultures of the typhoid bacillus, which has the composition $C_7H_{17}NO_2$.

The question of the etiology of croupous pneumonia has received much attention during the past year, and it is now evident that the bacillus of Friedländer, which has been cultivated for some years in the laboratories of Europe under the name of "pneumococcus," is not entitled to this distinctive appellation. On the other hand, evidence is accumulating that a micrococcus which I have described under the name of *M. Pasteuri*, and which is found in normal human saliva, is far more frequently found in the exudate into the alveoli during the acute stage of croupous pneumonia than is that of Friedländer. I first experimented with this micrococcus in 1880, and isolated it in pure cultures in 1881, but it was not until January, 1885, that I discovered its presence in pneumonic sputum, and made inoculations in rabbits with the material.

The record of my first successful experiment, published in the *American Journal of the Medical Sciences* for October, 1885, is as follows: "January 2, 1885. Inoculated two rabbits subcutaneously with pneumonic sputum, collected with great care by my friend, Dr. Rohé, and brought to me at once—from a white male patient, aged nineteen, in the seventh day of illness, second day of bloody expectoration. Both rabbits were found dead on the morning of January 4th. In both the pathological appearances were identical with those constantly observed by me in rabbits killed by the subcutaneous injection of my own saliva, viz., extensive inflammatory œdema extending from point of injection, enlarged spleen, and presence of oval micrococci in blood and in effused serum in the subcutaneous connective tissue." In the same paper I say:

"It seems extremely probable that this micrococcus is concerned in the etiology of croupous pneumonia, and that the infectious nature of this disease is due to its presence in the fibrinous exudate into the pulmonary alveoli.

"But this cannot be considered as definitely established by the experiments which have thus far been made upon lower animals. The constant presence of this micrococcus in the buccal secretions of healthy persons indicates that some other factor is required for the development of an attack of pneumonia; and it seems probable that this other factor acts by reducing the vital resisting power of the pulmonary tissues, and thus making them vulnerable to the attacks of the microbe. This supposition enables us to account for the development of the numerous cases of pneumonia which cannot be traced to infection from without. The germ being always present, auto-infection is liable to occur when from alcoholism, sewer-gas poisoning, crowd poisoning, or any other depressing agency the vitality of the tissues is reduced below the resisting point. We may suppose, also, that a reflex vasomotor paralysis, affecting a single lobe of the lung, for example, when induced by exposure to cold, may so reduce the resisting power of the pulmonary tissues as to permit this micrococcus to produce its characteristic effects.

"Again, we may suppose that a person, whose vital resisting power is reduced by any of the causes mentioned, may be attacked by pneumonia from external infection with material containing a pathogenic variety of this micrococcus having a potency, permanent or acquired, greater than that possessed by the same organism in normal buccal secretions."

The extended researches of Fränkel and of Weichselbaum show that this micrococcus is very commonly, if not constantly present in the exudate of croupous pneumonia, and both of these investigators are inclined to attribute to it a specific pathogenic rôle in connection with the malady in question. Fränkel's first paper was published in 1861. In this paper he says:

"Finally, as regards the relative frequency of the two hitherto investigated microbes, in cases of pneumonia, no positive statement can yet be made. Nevertheless, I am inclined to regard the lancet-shaped pneumonia-coccus, which is identical with the microbe of sputum-septicæmia, as the more frequent and the usual infectious agent of pneumonia, on the ground that this organism is so much more frequently found in the sputum of pneumonic patients than in that of healthy individuals. This conclusion is further supported by the circumstance that it has not hitherto been possible to isolate directly from the rusty sputum, Friedländer's bacillus."

Weichselbaum reports that he has found this organism in ninety-four cases of pneumonia, eighty of which were primary, and fourteen secondary. On the contrary, he found Friedländer's bacillus in only nine cases; in three of these cases it was associated with the diplococcus above referred to, and in only three instances was it obtained alone in pure cultures.

Weichselbaum arrives at the conclusion that pneumonia may be induced by several different organisms, but that the diplococcus which I have called *M. Pasteuri*, a name, by the way, which none of the German authors have been willing to accept, is by far the most frequent cause of genuine croupous pneumonia.

Whatever may be the final conclusion with reference to the specific etiological *rôle* of this or other microorganisms in pneumonia, we must recognize the importance of secondary causes which control the endemic and epidemic prevalence of the disease, and these have recently been worked out in a very satisfactory manner by a distinguished member of our Association, Dr. Baker, of Michigan.

Among the most important investigations of the past year are those of Councilman, of Baltimore, and Osler, of Philadelphia, with reference to the presence of microorganisms in the blood of malarial fever patients. Both of these observers confirm the discovery of Laveran, who, in 1880, announced as the result of extended researches made in Algeria, that blood drawn from the fingers of such patients, during a febrile paroxysm, contains a parasitic infusorium, which presents itself in different phases of development, and which in a certain proportion of the cases was observed as an actively motile flagellate organism. Osler and Councilman

have found all of the forms described by Laveran, and the last-named observer reports that in recent researches in which blood was obtained directly from the spleen, the flagellate form was almost constantly found. Whether the amœboid "plasmodium" found by Marchiafava and Celli, of Rome, represents an early stage in the development of this organism, or whether it simply represents a change in the red blood-corpuscles, which occurs also in other diseases, as is claimed by Mosso, has not yet been definitely determined. It is somewhat curious that just when we are receiving satisfactory evidence of the presence of the parasite of Laveran in the blood of malarial fever patients, the bacillus of Klebs and Tomassi-Crudelli, which appeared to be dead and buried, has again been introduced to our notice by the distinguished German botanist, Ferdinand Cohn. In his paper published in June last, he gives an account of the researches of a young physician, named Schiavuzzi, who has made researches in the vicinity of Pola, a malarial region in Istria. The method followed was that of Klebs and Tomassi-Crudelli, viz., examination of the air and water in malarial localities, and inoculation experiments in rabbits.

The bacillus was constantly found in the air, and the rabbits inoculated presented symptoms and pathological lesions believed to be identical with those of malarial fever in man. I cannot at the present time go into a critical discussion of the evidence presented, but would refer you to an experimental research made by myself in New Orleans in the summer of 1880, in which I repeated the experiments of Klebs and Tomassi-Crudelli, and arrived at the following general conclusions:

"Among the organisms found upon the surface of swamp-mud, near New Orleans, and in the gutters within the city limits, are some which closely resemble, and perhaps are identical with, the bacillus malariæ of Klebs and Tomassi-Crudelli; but there is no satisfactory evidence that these, or any of the other bacterial organisms found in such situations, when injected beneath the skin of a rabbit, give rise to a malarial fever corresponding with the ordinary paludal fevers to which man is subject."

I see no reason to modify the opinion here expressed, notwithstanding the endorsement given by Cohn to the results announced by Schiavuzzi. These researches relating to organisms in the air and water, and experiments upon rabbits, especially in the hands of an inexperienced investigator, cannot have any great scientific value in the elucidation of an etiological problem. The sources of possible error are too numerous, and the method is in any case inadequate for the complete solution of the problem. It is essential that the infectious agent, especially one so easily demonstrated as this bacillus, be proved to be present in the blood or tissues of malarial fever patients, and in the absence of such proof, experiments upon rabbits and researches in the air of malarial regions can have but little weight. It may well be that in the swampy districts of warm climates, where malarial fevers prevail, one or more species of bacillus will be found in the air or in the water, which are absent from the drier air and running waters of non-malarious uplands, but this is simply an interesting fact in natural history relating to the distribution of organisms of this class, and by itself cannot be accorded any value in a consideration of the important question of the etiology. The method of research pursued by Laveran, by Marchiafava and Celli, by Councilman, and by Osler, is the true one, and none of these gentlemen have encountered the bacillus of Klebs and Tomassi-Crudelli in their extended researches. On the other hand, they are in accord as to the presence in the blood of a peculiar flagellate organism, and of certain spherical and crescentic bodies which are believed to represent different stages in the life-history of this infusorium.

If the time permitted, I would be glad to continue this review of the recent progress made in the interesting field in which I have been for some time an enthusiastic worker, but your patience would be exhausted long before I could reach the end of the story.

The importance attached by the profession to studies of the nature of those referred to is well stated in a recent editorial in the London *Lancet*, relating to the International Hygienic Congress recently assembled at Vienna. The editor says: "The Vienna meeting will serve the double purpose of indicating the necessity of skilled investigation of the causes of disease, and of encouraging statesmen to rely upon work of this character, rather than upon collective opinion.

"The excellent work which is carried on in the continental laboratories has undoubtedly had its effect in teaching the value of exact knowledge, but there was none the less too great a desire at Vienna to record by resolution the opinion of numbers, regardless of their fitness to exercise any proper judgment upon the points at issue."

Before concluding my address, it becomes my sad duty to remind you of the losses which our brotherhood of sanitarians has sustained during the past year, to speak of those whose voice will no more be heard among us, who have joined the great army of the dead. Happily for us they have left a record behind them, of honest endeavor and earnest words spoken in behalf of their fellow-men. Their names are not simply recorded in the list of our members, but also in our hearts; and a permanent record of their contributions to sanitary science will be found in the papers to which their names are attached, which are contained in the volumes of our transactions.

And now, Gentlemen of the American Public Health Association, having already exceeded the limits which I had intended to place upon the length of my address, nothing remains but to thank you for the kind patience with which you have listened to me, and for the high honor which you conferred upon me at Toronto, in electing me your President for the present meeting.

ORIGINAL ARTICLES.

A DEATH DURING, AND A DEATH BEFORE, THE ADMINISTRATION OF ETHER.

BY D. HAYES AGNEW, M.D.,
PROFESSOR OF SURGERY IN THE UNIVERSITY OF PENNSYLVANIA.

ON the 3d of August, 1887, I was called to visit by appointment, Mr. R. D., æt. forty-five, at his hotel in Philadelphia, for the purpose of removing two ulcerating hemorrhoids, which had resisted the

ordinary means of treatment for several months, and were causing the patient great discomfort. Dr. White was asked to accompany me and administer the ether. There were no ascertainable reasons why an anæsthetic should not be given, as neither the heart nor kidneys were diseased, and the patient appeared to be in good health. Only one year previous I had divided and stretched the sphincter ani of Mr. D. for fissure, administering the ether myself, and after anæsthesia, entrusting the agent to a friend of the patient, who was present, for the brief time required for the operation. About six ounces were taken at this time.

At the time of the last operation nothing occurred during the early stage of the inhalation, other than what is witnessed every day when ether is exhibited. In the course of fifteen minutes the patient, though somewhat rigid, was placed across the bed. One of the tumors was dragged down, transfixed, and ligated with a double ligature. When about to seize the second, the breathing, which had been strong and free, suddenly ceased. The operation was immediately suspended, and the usual methods for resuscitation instituted. These consisted in examining the throat for the probable presence of some obstructing cause, dragging the tongue forward, the use of artificial respiration, flagellating the surface with the end of a wet towel, ammonia to the nose, partial inversion of the patient, and, finally, the passage of the electro-galvanic current through the phrenic nerves. Notwithstanding these measures were persisted in for at least three-quarters of an hour, during twenty-five minutes of which time the pulsations of the heart could be recognized, not a single effort of natural respiration occurred. The man was dead.

The post-mortem made by Dr. Formad, revealed complete collapse of the lungs, marked traces of an old meningitis, attributed to a former sunstroke, and what satisfactorily explained the sudden termination of life, the rupture of a calcified vessel in the floor of the fourth ventricle, the recognized physiological centre of respiration; all the vessels comprising the circle of Willis were in a similar state of atheromatous degeneration. It was evident, therefore, that the increased vascular tension of these cerebral vessels, caused by the ether, determined the lesion, a result which might have followed any unusual excitement, mental or physical. The heart and kidneys were healthy.

The question naturally occurs, Could this sad disaster have been prevented? Certainly, in view of all the circumstances, it could not. *First.* The ether was that known as Squibb's. The can contained eight ounces; two ounces remained in the vessel; two ounces, it is fair to suppose, had been retained in the folds of the towel, leaving four ounces, or, at most, less than five ounces which had been inhaled. *Second.* There was a sufficient admixture of atmospheric air with the respired vapor, as the anæsthetic was administered from an ordinary towel folded into a cone and with an opening at the apex. *Third.* The position of the administrator, Dr. White, that of reclining alongside of the patient, with the face of the latter in full view, would have enabled him to detect at once any signs of approaching danger, which, from his long experience in giving anæsthetics, would have been quickly recognized; and, *last*, the impossibility of being able to ascertain during life, the state of the bloodvessels of the brain disclosed by the autopsy, and rendered very improbable in a man of forty-five years of age.

After forty years of surgical work, often, too, of the gravest character, and sometimes requiring prolonged anæsthesia, without an accident in a single instance, I had come to believe that the exhibition of ether, unless recklessly administered, was entirely free from danger. Several deaths, I am aware, have been reported, from time to time from its use, even in the hands of the most careful operators, but I never could divest my mind of the idea that in these cases there was some undetected element involved more influential in the issue than the ether, as in the present instance.

The following case, which came directly under my own observation only a few weeks before the death of Mr. D., is in point.

I was called to one of our suburban towns to see a case of strangulated hernia. The patient was a female about seventy years of age. The intestine had been incarcerated for three days. Her pulse was quite good, and there were no signs of collapse. I noticed the pupils were much contracted, no doubt from the opium which had been administered. I directed her medical attendant to place the patient properly in bed, and prepare a cone for giving the anæsthetic, while I made my preparation for the operation. This was done. In a few minutes I was ready to proceed, and then told the doctor to give the ether. While reaching for the bottle, and before removing the cork, the patient gave a sudden convulsive movement, at the same time ejecting a large amount of stercoraceous matter from the stomach, and expired in a moment. Had the etherization been commenced, or the operation begun, before the death of the woman, the fatal result would have been charged to either the anæsthetic or to the knife.

THE ORIGIN OF SOME DISEASES.[1]

BY EZRA M. HUNT, M.D.
SECRETARY NEW JERSEY STATE BOARD OF HEALTH.

THE class of diseases variously known as contagious, transmissible, or communicable has ever attracted the most earnest consideration of medical men and of all sanitarians. By the quickness and often obscurity of their invasion, by the malignant type they too often exhibit, and by the large areas over which they extend, they not only make large demands upon the skill of the profession, but arrest the attention of all mankind.

The study of epidemiology has enlarged their numbers and now shows us that many diseases once regarded as constitutional or septic are in reality specific and pathogenic or mostly parasitic.

[1] Read before the American Public Health Association, at Memphis, November 8, 1887.

The prevention and limitation of this class of maladies must ever therefore largely occupy the attention of all of those who study the causes, the courses, and the results of disease.

The study of their etiology is always a *fundamental* inquiry. While to a degree it is possible to treat a disease skilfully without knowing its causes, it is always more satisfactory and generally more skilful to know something of the causes.

But what do we mean by etiology or causes, surely not always, not generally, the beginning or efficient or final cause? Prof. Semmola, of Naples, at the recent meeting of the Medical Congress at Washington may have startled some by these words, "Medicine, like all other sciences, never demands why, well knowing that the *first* causes of things are inaccessible, and that to every scientist it should *suffice* to know in which physical and chemical conditions this or that phenomenon manifests itself, so that he can modify and govern it at his will." This is true. The best that we can generally hope is to find the *conditions*, physical, chemical, or biological, in which phenomena manifest themselves. The word *cause* is often in the same sentence used in two or three different senses. In our etiology we must remember that by cause we mean mostly the "conditions under which phenomena manifest themselves." Also that these conditions (thus called causes) mean the modifying influences present in the host or person, and the modifying influences of surroundings much oftener than they mean anything in the specific entity which we are so often calling the germ, and then calling it the cause.

While reaching back toward the beginning, even though we seldom reach the starting-point, we do come to see how it is true of every disease as of every living thing, that it must have had a beginning. It is not a mere platitude to say that there was a day when the first case of smallpox occurred.

Nor does it necessarily belong to the sphere either of creation or spontaneous generation. A case of some new disease may put in an appearance, as did cholera on the Ganges, or a case of a known and existing disease may happen that is not derived from another precisely like it, but occurs because analogous conditions to those which gave rise to the first case of the kind have again occurred, and produced a pronounced deviation.

So it is possible that at the same time, as to some one disease, we may have cases of a communicable disease which have arisen from previous cases, and other cases that have resulted from a combination of influences or conditions such as gave rise to the first case. There are at least two reasons why the old dictum of *omne vivum ab ovo* cannot now be applied with the precision or with the finality with which it used to be quoted.

I. There is such a thing as *evolution*, which, while recognizing an original type, also recognizes departures from the normal which may have come to be so representative and paramount, as to constitute *newness* in all essential particulars. Since we have come to recognize that many diseases are but developments and cultures of microphytic or microbic life, we very appropriately turn to the facts of botany, not only for illustration, but for verification of our theories. And what a change has taken place in *its facts* since the days of Linnæus? We no longer cling to the divisions of orders, genera, and species so closely laid down by him. We recognize two forces—nature or heredity, and environment. A plant inherits a likeness which it *tends* to retain, but it is often so modified by environment, as greatly to change, and so sometimes as even to *lose* its identity. Environment comes to predominate over heredity. The horticulturist often takes a plant which he has found to be subject to variations, and fixes and perfects it in some one of them by cultivation.

Prof. Huxley has recently contributed to the Linnæan Society a paper on the classification of gentians, in which he claims that gentians are all specialized—that is, become gentians from some other form. *Permanency* of type has so many exceptions, that *variations* of type and the power to give fixity to some of these variations by means of cultivation or environment, must be accepted as a doctrine and a fact. Species and genera have variations, sports, modifications not dreamed of by the earlier botanists. Some of these departures are so marked and so predominant as to obscure the relationship and so far *ignore* it as to have individuality of their own. If, as we know, cultivation or surroundings can change a poisonous plant into a mild one, or can wholly modify it, it is not remarkable that a microphytic disease should lose its apparent identity, and at length in a new culture-medium or under special conditions become specialized. It is a law abundantly illustrated in the vegetable world, that environment causes variations, and that some of these variations tend to fixity of type, while others do not. All the wonderful facts of evolution show full well that we may in this way have what in respect of *symptoms and treatment* is a new disease.

Yet it is not a *de novo* origin in an absolute sense, or, if practically *de novo*, it is not *de nihilo*.

It is, that a series of changes has been evolved by environment, by conditions in persons or in surroundings, until the result of some of these changes becomes self-assertive and prevails over its heredity so as to secure for a longer or shorter period a fixity of its own.

This fixity may be a new disease.

II. The history of hybrids, the so-called accidents of their occurrence; and the fertility of hybrids is such that we are forced to look upon them as new forms of life—as becoming established into an autonomy or individuality of their own.

The horticulturist in some of the wonderful productions of new plants in the last score of years has come to be familiar with hybridisms that wholly obscure origin and that come to have a fixity of their own as much as some new diseases with which we have to deal, or as much as cases of disease which we cannot trace to a contagious source.

All this has been greatly emphasized by what we have come to know as the fertility of hybrids.

Not over thirty years ago the general view of botanists was that hybrids were sterile.

But so numerous are the exceptions and so abundant have been the results of new cultivations that this view is fully disproven. Says a recent botanical authority, "Among plants there are many instances of hybrids between species being perfectly fertile, and continuing so for an indefinite period. Experiments during the last twenty-five years have increased the number of such fertile crosses manifold. . . . Fertility of hybrid plants is the rule and sterility the exception. So far as plants are concerned, there is not the slightest ground for considering sterility as a distinctive bar, separating species. These hybrids come to have a specificity of their own so different from the parentage as to be unrecognizable, and so specialized as to be permanent. As another expresses it, "The hybrid becomes an individual not responsible to its species."

Nor is this confined to plants with spores. Some of our most skilful horticulturists are now producing varieties of *ferns* by hybridism, and every now and then some so-called chance growth or sport shows wide departures.

The bearing of all this on the parasitic forms of disease is not far to seek. If, as now seems so nearly proven, so much of disease has to do with elementary and minute forms of vegetative life, it is easy to see how the facts of evolution and hybridism have a bearing upon the appearance and propagation of disease. The light of the botanical world and its marvellous revelations as to the actualities and possibilities of the origination of new forms distinct from the parentage, penetrates the hidden sphere of disease-origin, and shows how some diseases cease, how others arise, how some lapse back to their heredity, while others are made permanent or specialized by their environment and how others still are hybridized into specific forms and acquire a fertility and fixity of their own. This range of deviation is so wide as to account for very many of the anomalies of disease and for the organization, cessation, or modification of form.

In reply to my inquiry on this particular point Mr. Mehan, the distinguished botanist of Germantown, Penna., writes me: "All hybrids, that we know by actual experiment *are* hybrids, are fertile as far as I know, and reproduce their originals just the same as if they were 'original' species. I think almost every botanist of note believes in abundant, fertile hybrids in nature. Coming down nearer to your own line of thought, I believe it is conceded that all lichens are hybrids between fungi and algæ. It is tenable that new forms of disease are continually coming into existence, which can only arise from new forms of disease-producing plants (microscopic fungi) being evolved from older species; but I do not know whether there is an opinion that these new forms are the result of hybridization or of that natural law of change which seems to be a constituent part of existence—in vegetation at least."

In a paper read by Dr. M. W. Taylor, before the Epidemiological Society of London, April 13, 1887, he notes the fact that many of the fungoid varieties were but "conversions of elementary states of pencillium and oïdium. It is also maintained by Zopf (the botanist), that there may be a *pleomorphism* amid pathogenic microörganisms, and that there are stages of intermediate forms resulting from the nature of the nutrient media." (See *Lancet*, May 7, 1887, pp. 933, 934.)

The view is, we believe, gaining ground, that harmless microphytes may become pathogenic, and that the different forms of microörganisms present have relation to each other, and that the culture-medium in disease, viz., the person and his surroundings, determines the character of the microörganisms full as much as the microörganisms determine the character of the disease.

If the views as to the microphytic origin of most of the communicable diseases are correct, the study of the laws of this evolution and hybridism is vital. We believe it is in this direction that we are to account for the origin of new diseases, or for such variations in type as obscure or destroy identity. If we can, through this study, arrive at the evidence that in this sense many diseases begin, we have a new department of study, in that we are called upon to define with accuracy how and why this origin takes place, in order that we may thwart or circumvent the conditions.

If it is the result of *evolution* through a long series of changes, it behooves us to study the normal, and to watch and record all the gradations by which the unfriendly result is attained, so that, at some stage, we may intercept the progressive and threatening changes that are occurring, or ascertain what condition of the person, or what condition of surroundings constitutes the influence which brings about the change or provides the fertilizing medium for the disease and causes it to break forth. If it is the result of a *hybridism* which occurs spontaneously or rapidly, we need to study precisely what forms of vegetative life thus incline to coalesce, under what condition the union occurs, and how their conjunction, development, and fertility are to be interrupted.

If special conditions of some parts—as the throat, for instance—or certain conditions of the secretions, furnish a special soil or culture-fluid for the propagation of low forms of vegetative life, this is to be studied with exactness.

In each of these lines the same methods of technical study and close record and analysis of facts by competent observers, which has prevailed in the study of minute plant life by botanists, and which has obtained in many other sciences, will, in this comparatively new field of biological and botanical research, accomplish equally valuable results. In it we are attempting to find out how much, and under what circumstances microörganisms imperil human life.

The practical value of such an inquiry is apparent, for sooner than is the case with most of the studies of nature, the results will be applicable in the prevention and treatment of disease. It will be a great gain if we can come to know that, either under the laws of evolution, or as the result of admixture or hybridism, symptoms and pathological effects become specialized so as to constitute a new disease which maintains its type.

As examples of how proximity of different diseases

may modify symptoms, we have many suggestive facts in the history of disease. Yellow fever is believed by many to be a mongrel, born on the high seas by admixture of the jungle fever of Africa with the typhus of the pent-up hold of the filthy vessel. It is not certain that typhoid fever was not once nearer to typhus, until it came to be called abdominal typhus, and then to have modifications because surroundings and the acquired power of self-propagation gave it an autonomy of its own.

It is not even now certain that there are not grades of cesspool and other adynamic fevers that will some day declare another well marked departure from what we now call typhoid, and come to have an individuality of their own.

It is not certain that when Sydenham treated scarlet fever and measles as one disease that their lines of difference were as well marked as now.

Diphtheria so often seems to have a localized origin, and common forms of sore throat are so often seen to pass away from their general into a special type, that it will not be strange if we can come to the law of departure. (See views expressed in the *Sanitary Reeord* of August 15, 1887, p. 88, and the contribution of Dr. Wordin, in the *Connecticut State Health Report for 1887*, that diphtheria results from the special virulence of a micrococcus which is not specific, but present in forms of foul or septic sore throat).

While typho-malarial fever has no pathognomonic lesion to distinguish it from the ordinary typhoid, yet we do know it has symptoms to distinguish it.

The advances of biological investigation have put us in regions of new possibilities, that do not involve spontaneous generation, but yet do render probable what is equivalent to the *de novo* origin of cases of disease which afterward are chiefly communicated by the first and succeeding cases.

Having settled that such origins do take place, we shall then pry into the secret of the laws of combinations and the conditions which favor the evil evolution or the facts of hybridism, and seek to combat these by starting similar processes in opposite directions, or by sterilization, neutralization, disinfection, and all the details of radical sanitation.

Such a view of the occurrence of old or new diseases and of the reasons for fixity in some and changing forms and types in others leads to several practical results.

1. The study of parasites, or germs, as they are called, is only one of the methods of informing ourselves as to the phenomena of disease, and in itself is not likely to be the key to rational and successful treatment.

2. Our attention should be directed far more than now, to the study of conditions and circumstances under which new forms appear; to the influence of persons and surroundings, instead of to the mere finding of a specific form. The latter would, of course, be most valuable as one of the facts in the chain of evidence, but we should not, as now, seek so much to look to it as the cause of disease as to inquire what conditions have *caused* this or that particular microphyte to be present.

3. We should be able to account for the occasional occurrence of a disease independent of any previous case, and for changing types of disease and new diseases, and would come to treat diseases less by their names and more in view of their type and the effect of surroundings upon them.

4. The tendency of all this is to magnify the importance of close observation and to lead us to feel that success in warding off disease and in treating it when it appears, depends mostly upon close observation and that experience which is derived from actual practice.

If we are looking to the biological laboratory for the natural history of disease, or to the chemical laboratory for the application of remedies, we shall surely fail. It is not so much that we need to find the specific germ, or the specific methods. The world is always looking after specifics. But the science and art of sanitation has far more to expect from a study of the conditions of persons and surroundings under which diseases, or types, and modifications of disease manifest themselves, as also from a study of the prevention or obliteration of such conditions, than it has to expect from the finding of microphytes as the source of disease, and seeking to cure disease by expelling microörganisms or attenuating them.

Our only design in this paper is to awaken inquiry as to modes of accounting for the localized origin of disease, without any antecedent case, on the proposition that the laws of evolution, environment, hybridism, or modification by culture, give rise to diseases so different from their prototypes as to have individuality and permanency of their own.

Because such inquiry is relevant to prevention, there is good reason to believe that by ascertaining the laws of these transformations and modifications of type and of the origination of special varieties, we shall ere long find new means for the prevention or limitation of many diseases.

THE PHYSIOLOGICAL ACTION OF THE OIL OF TURPENTINE.

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NOTWITHSTANDING the fact that the oil of turpentine has been used empirically for many years with great success in certain morbid conditions, but little effort has been made to study its relations with the animal organism other than by the bedside. Kobert[1] and Hoppe[2] have made studies of its action to a limited extent, as has also Azary, an Hungarian investigator; otherwise nothing has been done worthy of remark.

When from one to two drops of the oil of turpentine, having first been thoroughly stirred up with about fifteen cubic centimetres (four drachms) of water, is injected into the jugular vein of a dog weighing thirty-four pounds, there is produced immediately an increase in the number of the cardiac movements which lasts for an indefinite period. There is, how-

[1] Centralblatt f. med. Wissenschaften, 1887, p. 129.
[2] Journal für Pharmacodynamik, Bd. I. p. 105.

ever, no rise whatever in the arterial pressure, the vasomotor system being absolutely unaffected.

If a larger dose, amounting to five or ten drops, be used, the same increase in the cardiac pulsations occurs, but is followed in the course of from two to three minutes by a very marked slowing of the pulse, which is decreased in number considerably below the normal. Along with this primary increase in rate, there is a slight rise of arterial pressure which does not exceed ten millimetres of mercury, and which returns to the normal on the appearance of the slowing of rate mentioned. When a still larger dose (30 ♏) is administered in the same manner, the primary increase in rate lasts but for a moment, and is immediately supplanted by a very marked decrease in rate resembling that already mentioned. The arterial pressure still remains virtually unaffected. Doses of as much as two drachms of the pure oil produced a very marked fall of arterial pressure with great cardiac depression, and in one case death, in which respiration continued twenty seconds after the heart had ceased to beat. From the character of the pulse waves it was evident that the fall of pressure was due to cardiac failure rather than to any vasomotor action, and this was still further proved by the fact that asphyxia produced a momentary rise of pressure, a considerable rise being impossible in the depressed condition of the heart.

DOG, WEIGHT 34 LBS.

Table of experiment showing slight effect of two drops of turpentine, on arterial pressure.

Time.	Drug.	Pressure.	Pulse.	Remarks.
3.19		114-146	190	
3.19.10		114-150	184	
3.19.20	2 drops in 15 c.c. aq.	120-148	204	Injection begun. Injection ended.
3.19.35		118-148	224	
3.19.45		110-140	210	
3.19.55		112-148	210	
3.20.30		116-148	216	
3.21		120-146	240	
3.21.10		122-150	235	
3.22		123-152	210	

In order to determine whether the cause of the primary quickening of the heart-beats was due to palsy or depression of the vagi, these nerves were galvanized, and were found capable of reducing cardiac movements as completely as in the normal animal, and this proof of their integrity is still further supported by the fact that later on the drug was shown to be a stimulant to these nerves. It, therefore, is evident that this increase in pulse-rate must be due to a direct action on the heart muscles or to a stimulation of the accelerator nerves. That the action is on the heart itself, is pointed to by the fact that the drug applied directly to the heart of the frog produced an increase in rate, and this was proved by cutting the spinal cord in the dog, thereby making section of the accelerator nerves. Under these circumstances, the increase in rate after small doses still continued.

That the slowing which comes after the use of from five to thirty drops is due to inhibitory stimulation of centric origin is proved by the fact that section of the pneumogastric nerves immediately causes the typical rapid pulse, produced ordinarily by such a section. That the slowing is not due to cardiac depression is proved by the strength of the individual pulse waves and the constancy of the arterial pressure, and by the typical curves, after large doses, of vagal stimulation. These experiments were repeated so often and with such varying doses, but with an equally great constancy of the same result, that there can be no doubt of their truth.

The results are very much opposed to those obtained by Kobert and Hoppe, who found that the drug very powerfully affected the vasomotor system, at first causing a rise of pressure and later a fall. They found, however, as did the writer, that the heart is stopped in diastole, or, in other words, paralyzed. In their experiments, however, the vagi seemed to be unaffected.

I do not know in what lies this difference in results, unless it be due to some difference in the character of the oil used. The oil used in my own experiments was obtained from one of the most reliable druggists in Philadelphia, who was informed that the best was wanted, and also of the purpose for which it was to be used. That the absence of effect on the vasomotor system was constant, was proved not only by the use of doses of various size to various dogs, but by the administration of ascending doses of from two to thirty drops of the oil to individual dogs. In all cases the results were identical. These same results were reached in curarized and uncurarized animals, so respiration could have had no influence.

On the nervous system of the frog, the drug exerts a very decided influence when used in large amount. If as much as five drops be injected into the posterior lymph-sac of a medium sized frog, reflex movements are rapidly decreased, so that they occur only on decided stimulation. When they do occur, they are, however, very strong. Mechanical stimuli first fail to produce contractions, after this even burning fails to produce any movements, or, if any occur, they are very weak. That this failure of reflex is due to sensory involvement and not to any influence on the motor portion of the nervous system, is proved by the fact that powerful contractions take place in the limbs if the batrachian so desires, although the foot may be destroyed without calling forth any movement. Later, the frog lies on its back when so placed, but is still capable of originating powerful muscular contractions.

Finally, respiration becomes less and less, motion fails as well as sensation, and the animal dies.

The question as to whether the failure of sensation is due to the action on the nerve trunk or spinal cord was decided by tying a ligature around an entire limb, except the nerve trunk, and injecting the poison into the limb below the ligature, the spinal cord being therefore protected. Under these circumstances there was a decided lessening of reflexes even to severe stimuli, such as burning. At the same time it was proved that the sensory part of the spinal cord does not escape, for, if the artery in

a limb be tied and the poison given into the body, reflexes are lost even more completely. When much smaller doses are used ($\frac{1}{10} = \frac{1}{4}$ drop) reflexes are at first increased, then slightly diminished, the increase being due to a stimulant action on the spinal centres.

Summary.—1. The oil of turpentine in small doses, resembling those ordinarily given in practical medicine, produces an increase in the number of the cardiac beats due to a direct stimulant action on the heart.

2. In larger doses it produces distinct slowing of the pulse, due to a stimulation of the pneumogastric or inhibitory centre.

3. That its influence on the vasomotor system, if at all, is very slight, either with large or small doses.

4. That poisonous doses (5 c.c. to 10 c.c.) (♏. 80 to 160) produce death by cardiac failure when injected directly into the jugular vein.

5. That the drug in small doses increases reflex action somewhat, but in large doses decreases it, the increase being due to a stimulation of the spinal cord, and the decrease due to depression of the sensory side of the cord and afferent nerves.

MEDICAL PROGRESS.

CONDUCT OF THE THIRD STAGE OF LABOR AT THE ROTUNDA HOSPITAL, DUBLIN.—MACAN, in his address before the Section for Obstetrics of the British Medical Association, at its last meeting, described the method employed at the Rotunda as follows:

As soon as the child's head is born the left hand is placed on the fundus, the ulnar edge being directed toward the vertebral column with the palm of the hand looking directly in the axis of the uterus. The child's eyes are then carefully wiped, and a finger is passed into the vagina to see if the cord be round the child's neck. If uterine action does not soon expel the child, or if it should make any effort at inspiration, showing a want of aëration of its blood, then pressure is made on the fundus, which is generally sufficient to cause the body of the child to be expelled. If it does not do so, then gentle pressure is exerted on the child's head so as to push it backward against the perineum till the anterior shoulder descends with a well-marked jerk past the pubes, and then the delivery is readily terminated by pressure over the fundus. As the child leaves the uterus the left hand follows down the fundus, and causes any liquor amnii that is still in the uterus to be expelled. The position of the hand must now be the same as before the child was born, that is to say, the ulnar edge must be directed backward against the woman's spine, and the fundus should be pressed, if anything, forward against the pubes. The whole hand is now gently rotated, so that the tips of the fingers irritate the posterior surface and sides of the uterus, and more especially the points of insertion of the round ligaments. We can then follow and watch over the alternate contractions and relaxations of the uterus, and judge whether it is becoming unduly distended with blood or not. In the meantime the nurse steadily ties the cord as soon as all pulsations in it have ceased, and then the uterus is never for a moment out of the direct observation of the medical man in charge of the case. Should the child be bodily asphyxiated—that is, in a state of white asphyxia—the nurse should tie and cut the cord at once, and then take charge of the fundus of the uterus, while the medical man takes charge of the infant. When there is any considerable tendency to *post-partum* hemorrhage it becomes a very nice question whether the medical man should direct his efforts to resuscitating the infant or to stopping the *post-partum* hemorrhage. In any case of doubt the mother's care demands our first attention. If the discharge of blood remains moderate, and there is no accumulation taking place into the uterus, we use gentle friction at intervals for fifteen minutes, and then make firm pressure on the fundus in the axis of the brim during the acme of a contraction. This is nearly always sufficient to expel the placenta both from the uterus and from the vagina. The hand still, however, commands the fundus, and friction is again applied when we feel the uterine contraction beginning to relax. Should this interval of relaxation pass without any hemorrhage, then we wait for the next contraction, and apply the binder during its continuance. After the binder is applied the woman again turns on her back, and the hand is rubbed backward and forward over the fundus till it is found to be in such a state of permanent retraction as to exclude all danger of *post-partum* hemorrhage.—*British Medical Journal*, No. 1390, 1887.

IMAGINARY TUMORS OF THE TONGUE.—At a recent meeting of the Academy of Medicine of Paris, HARDY corroborated the observations of Verneuil, Fournier, Labbé, Laborde, and others, who had reported cases of imaginary tumor of the tongue in neurotic patients. Hardy had also recognized the disorder in those suffering from the late stages of syphilis, who had taken mercury for some time. In such patients there is generally a redness and slight swelling persisting for a long time after the acute stage has passed. The sensory nerves of these parts become altered in structure, and after all gross lesions have disappeard, sensations of pain and burning cause the patients to apprehend cancer. Inspection generally reveals no alteration beyond possibly a few mucous patches. The most useful treatment has been the long-continued administration of alkalies, with the local application of glycerine and carbolic acid. In syphilitic patients, whose sensory lesions generally diminish in acuteness as the disease progresses, these sensations of cancer grow less acute as time goes on, generally disappearing.—*L'Abeille Médicale*, October 10, 1887.

CONTINUOUS IRRIGATION, WITH HOT WATER, IN SEPSIS AFTER LAPAROTOMY.—DR. HAGGARD, of Nashville, reports a case in which, after partial removal of a fibroid tumor of the uterus, necrosis of the remainder and sepsis occurred, treated by continuous irrigation for a week with water at a temperature of 105°, and the free use of tonics. Recovery and cessation of all previous symptoms ensued. He adds the report of a case of laparotomy for cyst of the broad ligament, in which sepsis occurred, owing to the presence of necrotic tissue, whose removal was impossible in the exhausted condition of the patient.

Constant irrigation with hot water for twelve days

resulted in maintaining the elimination of noxious material, and the patient recovered. In both cases the usual methods of drainage failed.—*Southern Practitioner*, November, 1887.

A CASE OF CHARCOT'S DISEASE.—DR. PARK reports in *The Medical Press* for November, 1887, the case of a man, aged forty-seven, who has been bedridden for thirty years, presenting the peculiar deformities of Charcot's disease in a most extraordinary degree. Careful examination differentiates the case from *myositis ossificans*, and the peculiarly well-marked bony lesions are such as to render the case extremely interesting.

ANTIPYRIN IN THE TREATMENT OF DISEASES OF WOMEN.—MÉNIÈRE has found antipyrin useful in dysmenorrhœa, especially when attended by neuralgia, for which he gave the drug in a prescription containing 70 grains in 4 ounces of menstruum, of which, when the pains were moderately severe, he gave from 3 teaspoonfuls to 3 tablespoonfuls daily. In cases of ante- or post-menstrual crises, the dosage should be largely increased; and, if needed, the following may be given by rectal injection:

Infusion of valerian	ʒ 7½.
Decoction of poppies	ʒ 7½.
The yolk of one egg.	
Antipyrin	grs. 15 to 45.

To be made into an emulsion, and given warm.

Prompt and positive benefit has followed the use of this combination.

In dysmenorrhœa from obstruction to the flow, the following has proved efficient:

Aquæ chloroform.	ʒ 25.
Syr. digitalis	ʒ s¾.
Liq. ether	ʒ 3¾.
Antipyrin	gr. 75.

Of which a teaspoonful, or dessertspoonful, may be given hourly.

In some cases ergotine or hamamelis may be combined with antipyrin, as follows:

Aquæ tiliæ	ʒ 25.
Ergotin.	gr. 15.
Syrupi digitalis	ʒ 5.
Antipyrin	gr. 75.

Also

Aquæ tiliæ	ʒ 25.
Fl. ext. hamamelis virg.	♏ 30.
Syrupi digitalis	ʒ 5.
Antipyrin	gr. 75.

Linden-flower water and syrup of digitalis are not officinal in America, and any convenient menstruum, and the usual preparations of digitalis should be employed; the dosage is that usually employed with the ingredients.

Antipyrin may be given by hypodermatic injection in

Recently boiled water	ʒ 2.
Antipyrin	gr. 30.

of which 20 drops represent 3 grains of antipyrin; from 2 to 4 such injections are needed to secure the effect of the drug.

To render these injections as painless as possible cocaine may be added:

Recently boiled water	ʒ 2.
Cocaine hydrochlorate	gr. 1½.
Antipyrin	gr. 28½.

which may be injected deeply in doses of 20 drops.—*Gazette de Gynécologie*, October 15, 1887.

THE ANTISEPTIC TREATMENT OF PURULENT OTORRHŒA.—Quite remarkable results have lately been obtained by the strictly antiseptic treatment of purulent otorrhœa suggested by Dr. W. Chrystie. Specula have all been kept in a 1 to 20 solution of carbolic acid, after having been carefully washed in hot water, and cotton-carriers are in each instance heated to a red heat before being used. The external canal and fundus of the affected ear are first carefully dried out with ordinary absorbent or borated cotton sterilized holders, and the fundus is then mopped absolutely clean with 1 to 40 carbolized solution cotton tufts. This in turn gently dried out, and a powder consisting of iodoform 1 part, boric acid 6 parts, is then insufflated. By this method the duration of treatment has been shortened to two or three applications in many cases, and an average of about eight treatments is sufficient to cure most cases, even when of some years' duration. Thorough treatment of the always diseased naso-pharynx is carried out with great care in all cases of purulent otitis. —*Polyclinic*.

ICTERUS NEONATORUM.—SILBERMANN is quoted by the *Centralblatt für Kinderheilkunde*, No. 17, 1887, in an extended article, in which he concludes that:

1. Icterus of the newborn is a hepatogenic, absorption icterus.

2. The engorgement with bile occurs in the biliary capillaries and interlobular bile-ducts, which are compressed by the congestion in the portal vein and blood capillaries.

3. This engorgement with blood is occasioned by an alteration in the hepatic circulation, occurring shortly after birth, which is a part of a general change in the blood plasma.

4. This blood change is a *fermentæmia* (development of blood ferment).

5. Icterus is more pronounced the weaker the child.

6. The decomposition of a large number of red blood-corpuscles furnishes abundant material for the formation of biliary coloring matters, which, under the working of the ferment mentioned, continue to decompose for a long time in the hepatic vessels.

POISONING BY SULPHUR POMADE.—EICHBAUM of Leipzig, reports in the *Berliner klinische Wochenschrift* of October 17, 1887, the case of a man, aged thirty-seven, well developed, and previously healthy, who complained of headaches, giddiness, and attacks of partial syncope. Examination revealed the face pale, the forehead covered with cold perspiration, the pupils dilated and not reacting, the movements of the eyes free, the tongue protruded straight, tremulous, and coated. The heart sounds were a little dull, but no murmurs were present. The other viscera gave no signs of disease. Pulse 124, very small; respiration 16; temperature subnormal. There was some rigidity of

the muscles on the right side of the neck. The patient complained especially of occipital headache. There were no history or signs of syphilis or other well-marked symptoms.

By careful investigation and exclusion, a diagnosis of chronic sulphur poisoning from the use of pomade was made. This pomade consisted of—

Vaseline	100 parts.
Wax	5 "
Sulphur	10 "
Oil of roses	a few drops.

which the patient had used for some time to cure dandruff of the scalp, applying it every second day. The preparation of sulphur used was raw powdered sulphur, the oxidation of which, and its mingling with the perspiration, had formed sulphurous acid, whose absorption caused the poisoning. The cessation of the use of the pomade, the employment of hot baths, and exercise in the open air, speedily restored the patient to health. Eichbaum estimated that three and a half ounces of sulphur had been used by the patient.

EXCAVATING MYELITIS.—Hydro-myelia, syringo-myelia, and peri-ependymal myelitis are synonyms of the excavating myelitis on which MM. Jeffroy and Orchard recently read a paper before the Academy of Sciences, Paris (*La France Médicale*, Oct. 11th, No. 119). In two cases which they examined they found evidence of a slow inflammation, and they consequently reject the view that the cavities result from enlargement of the central canal from cystic degeneration of a hemorrhagic or gliomatous focus. These authors believe that the distribution of the vessels to the centre of the cord explains the anatomical disposition of the cavities. According to them, the course of events is first a myelitis affecting a special site in the neighborhood of the ependyma, and this inflammation causes an obstruction of the vessels supplying the gray matter around the ependyma, and consequently there results a softening which proceeds to excavation. The formation of the limiting wall to the cavity follows, or is posterior to these alterations, and its pathogeny is therefore the same as that of the wall of ancient foci of cerebral hemorrhage or softening. This interpretation differs from that of Grimm, who finds in the constitution of this vascular false budding membrane the characters of a tumor and the origin of the whole process.—*Lancet*, October 29, 1887.

THE ENGAGEMENT, IN THE PELVIS, OF THE UTERUS DURING THE LAST MONTHS OF PREGNANCY.—STAPFER concludes, from his clinical study of this subject, that this phenomenon is the result of a translation or transference of the entire uterus, and an elongation of the lower segment. It may proceed rapidly or slowly, definitely or temporarily. The degree of engagement may vary daily, particularly in the last weeks of pregnancy. The variations in this condition may be considerable, the fœtal head may remain as low as the perineum, or ascend to the superior strait. This uterine engagement is caused by the contractions and force exercised by the ligaments supporting the uterus, and by pressure; it is maintained by the tonicity of the ligaments and of the abdominal walls. It is favored by the empty condition of the adjacent viscera, by the absolute or relative volume of the fœtus, by the presentation, and by the elasticity of the inferior uterine segment, and of the membranes. The ascent of the fœtus to the superior strait renders changes in presentation possible, but rare. Any presentation, no matter how deeply situated, may be changed, if the forces producing it vary.—*Archives de Tocologie*, August 15, 1887.

THE TREATMENT OF THE THIRD STAGE OF LABOR.—FREUND, of Strassburg, in the *Deutsche medicinische Wochenschrift* of October 27, 1887, gives his conclusions regarding this subject as follows: In normal cases a judicious combination of the expectant and active method in treating the placenta and membranes is best. In removing the placenta the accoucheur should wait until the first signs of spontaneous expulsion appear. In cases where the placenta must be removed at once Credé's method of expulsion is the best. Freund has observed, in normal cases, that the uterus rises toward the umbilicus when it expels the child. He imitates, with one or both hands, this action in securing the expulsion of the placenta by gently pushing the uterus upward, and at the same time compressing it.

FORMULÆ FOR THE HIPPURATES OF CALCIUM AND IRON.—POULET, in the *Revue Générale de Clinique et de Therapeutique* of October 20, 1887, gives the following formula:

℞.—Acid. hippuric.	ʒ 25.
Calcii hydrat., q. s. for an alkaline reaction.	
Aquæ bullient.	2 quarts.
Sacchar.	5¼ pounds.
Tinct. citron.	ʒ 3¾.

The acid should be dissolved with the calcium hydrate in water heated to 176° F. until neutralization occurs; the other ingredients should then be added.

Syrup of hippurate of lithium should be prepared after the following:

℞.—Lithii carbonat.	ʒ 2.
Acid. hippuric.	ʒ 8¾.
Aquæ	1 quart.
Sacchar.	ʒ 37½.

This may be prepared as directed for the hippurate of lime. These syrups should be given pure, or mixed with water, before meals. The doses are two or three teaspoonsful or tablespoonsful daily for adults, and for children proportionate quantities.

Hippurate of iron may be made into pills by combining

℞.—Ferri sulphat.	ʒ 5.
Potass. carbonat.	ʒ 7½.
Acid. hippuric.	ʒ 10½.

The sulphate of iron should be first dissolved in boiling water and the solution filtered. The hippuric acid should be neutralized by the potassium carbonate and the warm solution filtered. The two solutions should be mixed, allowed to settle, and the fluid decanted. To the hippurate of iron formed ʒ3¾ of powdered gum arabic and as much honey should be added and the whole evaporated to a suitable consistency. Pills con-

taining 3 grains each should be made, of which one should be taken before each meal. This preparation is well borne by delicate stomachs. The hippurates may be given when a mild alterative is desired, to be used for a considerable time.

POISONING BY FISH.—A Spanish naval surgeon, Señor Don Antonio Juradoy Calero, writing in a recent number of the *Boletin de Medicina Naval*, describes the serious effects observed on board his ship, the gunboat *Magallanes*, from eating fish caught in the Bay of Buena Esperanza on the south coast of Cuba. No less than twenty-seven of the officers and crew were affected, most of them with gastro-intestinal disturbance of a more or less severe nature, the others with nervous symptoms. Those of the first group presented nausea, persistent vomiting, and diarrhœa, followed by coldness of the extremities, small and contracted pulse, and in some cases rigors; afterward a febrile condition supervened. The men were nearly all able to return to duty within two days. They complained, however, for some time afterward of irritation of the skin. In the more severe cases there were dilatation of the pupils, marked diminution of sensibility, complete anuria from spasmodic closure of the sphincter of the bladder, and in some cases convulsive movements of the muscles. Those whose symptoms were purely nervous were not attacked till some time later than those forming the first group, and they suffered much less severely, complaining chiefly of cutaneous irritation and headache, also of sleeplessness, which last continued more or less for three or four weeks. In both forms of the affection the termination was accompanied by a cutaneous eruption like urticaria, or in some cases by the appearance of a number of pustules on the skin. This, the author thinks, indicates that the proper treatment for poisoning by fish is to act on the skin. Besides, therefore, combating the gastro-intestinal and nervous symptoms with opiates, bromide, etc., he endeavored to produce diaphoresis by giving lumps of sugar containing twelve or fourteen drops of sulphuric ether, and exhibited an antispasmodic mixture consisting of syrup of orange, lime-flower and orange-flower water, and sulphuric ether. He allowed the patients to drink hot tea freely. —*Lancet*, October 29, 1887.

ENDOMETRITIS DURING PREGNANCY. — VEIT, of Berlin, in the *Berliner klinische Wochenschrift*, No. 35, 1887, does not accept the nomenclature *endometritis decidualis*, but proposes *endometritis gravidarum* as a proper name for this condition. He believes that in these cases endometritis exists prior to pregnancy, but first declares itself as the condition of pregnancy advances. The only treatment possible during pregnancy is that by rest and opium; but, as soon as the pregnancy is passed, the local and general treatment of the disease should be vigorously pushed.

THE TREATMENT OF THE ADVANCED STAGES OF UTERINE CANCER.—BETRIN, of Geneva, in the *Nouvelles Archives d'Obstétrique et de Gynécologie* of October 25, 1887, reports excellent results from the use of tampons dipped in equal parts of terebene (hydrated turpentine) and any convenient oil—almond, olive, etc. An abundant preliminary douche of a deodorizing solution of permanganate of potassium should be given, the parts dried with cotton, and from three to five tampons dipped in the mixture of terebene and oil should be placed in contact with the diseased tissues, and retained by a cotton tampon. The results in deodorizing and disinfecting the diseased tissues have been excellent, and no interference with the excretory functions has been observed.

NITRITE OF AMYL IN CHOLERA.—DR. PALM writes as follows of his use of nitrite of amyl in cholera, in the *British Medical Journal* of November 5, 1887:

During an epidemic of cholera in Japan, in 1879, it occurred to me to make a trial of the inhalation of nitrite of amyl in the treatment of the collapse of cholera. I did so upon the consideration that this drug appears to have an antagonistic action to the cholera poison in its effect upon the vasomotor nerves. The latter causes a dilatation and congestion of the vessels of the viscera, and a corresponding contraction of the more superficial vessels; while we have in nitrite of amyl an agent which, whatever its effects may be upon the visceral circulation, produces a marked dilatation of the superficial vessels, and must, therefore, to a corresponding degree, relieve the congestion of the visceral circulation. I found that cholera patients in a state of collapse could inhale it very freely before an effect was produced. My plan was to hold a bottle of it under the nostrils, directing the patient to sniff it up freely, and to repeat this frequently. Under its influence a small, thready pulse increased in volume, and where the pulse at the wrist was imperceptible it became perceptible. I have reason to think that in some cases it was the means of saving life. I regret that owing to the accidental destruction of my notes taken at the time, I can give no exact account of the number or details of cases in which it was made use of, but this hint may be of use to others who will have more opportunity than myself of further testing its value.

FACTORY LEGISLATION AND HYGIENE.—At the recent meeting of the Vienna Congress for Hygiene resolutions were adopted to the effect that children from fourteen to eighteen years should be allowed to work in factories for only a few hours daily, and not at all at night, and those under the age of fourteen should not be employed at all. A resolution was also passed that women should be allowed to work for only a limited number of hours during the day, and that they should never be employed at night. It was desirable that women should not be allowed to work in factories for several weeks after confinement, and that the State should take care that they did not lack the means of subsistence during this period of enforced idleness. The Section agreed in fixing from ten to eleven hours as a full working day, and a further resolution was passed that the observance of Sunday as a day of rest was one of the most urgent hygienic necessities for the working classes. The full advantages of this could only be secured if women were given the opportunity of doing their domestic work on the Saturday. A copy of these resolutions was sent to the Parliaments of all the States which took part in the Congress.—*British Medical Journal*, November 5, 1887.

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SATURDAY, NOVEMBER 19, 1887.

THE CROWN PRINCE'S DISEASE.

SIR MORELL MACKENZIE cables THE MEDICAL NEWS, from London, that "although there is no microscopic proof as to the nature of the present growth in the Crown Prince's larynx, it is in my opinion, no doubt, malignant. It is half an inch below and in front of the growth removed by me last summer, of which there is no return. There is, without doubt, limited perichondritis. The œdema which recently appeared was of an acute character, and had entirely passed off when I left San Remo. I think it, however, not unlikely to return in a chronic form."

The inference drawn by many from the nature of the present growth that the initial growth must have been carcinomatous, and that the correctness of the opinion to this effect entertained by some of the Prince's medical attendants at the period of Sir Morell Mackenzie's first summons is thus verified. The contrary opinion of Prof. Virchow, the eminent pathologist who examined the fragments removed by intralaryngeal procedure, seems justified by the fact mentioned in the above dispatch that there has been no recurrence at the point from which the first growth was removed.

Two points of great interest arise in connection with the present phase of the case. One is whether a benign growth has undergone malignant change; and the other, whether exsection of the diseased half of the larynx, for which preparations had been made at the period of Dr. Mackenzie's first visit, would have been better treatment than that adopted by the distinguished specialist. The best pathologists and clinicians, as a rule, assert that histologically benign growths have no tendency to become malignant. A few only think otherwise. It is a matter of clinical history in the practice of some physicians, that benign growths are sometimes followed by malignant ones "as the apparent result of long-continued irritation from the functional activity of the larynx." This may have been the cause of the unfavorable sequence in the case of the Crown Prince. Great disposition to inflammation has been observed from time to time, and functional activity has been noted as injurious.

As far as can be gleaned from the reports at present at hand, there seems as much to hope for from a laryngectomy now as there was six months ago. During these six months, however, the patient has lived in comparative physical comfort, without the sacrifice of his larynx, and without subjection to the immediate perils inseparable from submission to such a serious procedure. This gain is due to the stand taken by Sir Morell Mackenzie; and, whatever may be the ultimate outcome, his conservative treatment has certainly contributed to the welfare of his patient, and probably to the prolongation of his life. Even now it is said that laryngectomy may be safely postponed for weeks, and perhaps for months.

The prospects of a patient with carcinoma of the larynx are extremely unfavorable. Submitted to palliative and symptomatic management, including tracheotomy when called for by dyspnœa, his expectation of life may vary between the limits of eighteen months and four years, according to the character, location, and extent of the local disease and the constitutional vigor of the patient. Subjected to radical treatment by exsection of the larynx, he runs the risk of succumbing in a very short time to the immediate results of the operative procedure; but should that risk be successfully run, his expectation of life is lengthened by several years, possibly by many, provided there be no recurrence. Should recurrence take place within a few months, the operation will not have prolonged the patient's life. If the radical procedure is to hold out any fair prospect of success, it must be undertaken while the disease remains circumscribed in the structures of the larynx. Extension beyond the larynx increases greatly the immediate risks from the operation, and diminishes greatly the probability of ultimate recovery.

The question of laryngectomy becoming urgent at any period of the clinical history of a carcinomatous larynx, the decision must be made promptly. If the operation be declined, the patient must resign himself to the inevitable with what content he may. He cannot expect a rescue by changing his mind after the disease has extended beyond the larynx.

As to the procedure itself, it may be said that aseptic measures have removed from laryngectomy much of the immediate risks that formerly accompanied it. The records of operations performed during the past five years compare so favorably with those of earlier date that in a vigorous individual

with carcinoma confined to the larynx, and free from evidence of glandular involvement or carcinomatous cachexia, the operation may be regarded as advisable when performed with all the precautionary safeguards of modern improved surgical methods.

NEW METHODS OF TREATING PERTUSSIS.

WHATEVER methods are used in the treatment of this exceedingly troublesome disorder must depend, if used without empiricism, on some particular belief on the part of the physician as to the true cause of the disease. If he thinks that the condition is due to a neurosis, as did most of the older clinicians and do many of the present day, he must use remedies directed toward the alleviation of the nervous disorder. If, on the other hand, he has confidence in the theory, of much later origin, that the trouble is produced by the presence of some peculiar living entity, his manner of attack must be different in its primary object. While many other theories have been advanced as to the cause of this disease, we believe that these are the two which have been most generally accepted, and that at present the microbe theory of its causation finds the most general acceptance.

Arguing from this basis, PROF. BINZ, as long ago as 1870, claimed that quinine had a specific action in whooping-cough, provided it was given in very large doses in solution, so as to come in contact with the mucous membrane in its passage through the pharynx, and, in 1871, LETZERICH announced that the theory of Binz was correct, and that a peculiar microbe did exist.

Still later, HENKE, in the *Deutsches Archiv für klin. Medicin*, Bd. xii. p. 630, reported valuable results obtained by inhalations of quinine, and his reports have been followed by many others, among which may be named those of Parker, Perrera, and Sauerhering. In the hands of all these observers such practices were followed by good results, and their methods of administration only differed in the minor details, such as the proper strength of the solution to be employed, or of the variety of the salt, the hydrochlorate, sulphate, and tannate having been most generally found efficient. In some cases, however, the drug was given by the stomach in solution, in order to act on the mucous membrane of the mouth and pharynx, while in the hands of Henke, for example, the solution was used by inhalation from an atomizer. This latter method would certainly seem the more rational. The strength of the solution may vary from one to six grains to the ounce of water.

Among the other drugs used with a similar object have been salicylic acid, carbolic acid, resorcin, and oil of turpentine; none of them, however, has been as thoroughly tested as the cinchona alkaloid.

Those who support the theory that the disease is due to other causes than a microbe have employed, as every one knows, nearly every drug in the pharmacopœia, without deciding on any one as being much better than the rest.

Thus, Michael has used a very large number of powders made from various substances, notably quinine, which was used more for its action on the nasal mucous membrane than for any antiparasitic effect. Benzoin and bismuth were also employed with a moderate degree of success.

Still others have used local anæsthetics, the most important of which has been cocaine, and even here the reports are quite favorable.

The treatment of individual violent paroxysms is entirely different from that directed to the ultimate relief of the sufferer, and varies greatly with the circumstances of the case. Within the last year or two, inhalations of amyl nitrite have been used several times with marked success, in certain cases where the interference with respiration was severe enough to threaten death.

THE OSTEOGENIC FACTORS IN THE DEVELOPMENT AND REPAIR OF BONE.

IN our issue of October 22d we called attention to a paper by DR. WILLIAM MACEWEN, of Glasgow, in regard to the factors in the development and repair of bone. This paper is completed in the November number of the *Annals of Surgery*, and the discussion of the subject is continued in an additional series of propositions, which, to a considerable degree, tend to overturn the accepted views in regard to the *rôle* that the periosteum plays in the production of bone.

Proposition F states that the periosteum does not initiate the reproduction of bone. The proof of this is seen in the fact that if a subperiosteal excision of a healthy adult bone is performed, there will be little or no ossific reproduction. The periosteal tube collapses, and its position is afterward marked by a layer of fibrous connective tissue, which ultimately becomes absorbed. The osseous tissue which is developed under such circumstances is due to the entanglement of osteoblastic cells in the connective tissue, which is pulled out of the Haversian canals during the removal of the periosteum. When matured bone is irritated for a long time good bone is developed, and this is because formative osseous elements have been poured out on the surface of the bone from the Haversian canals during irritation of the bone, and have become entangled in the meshes of the periosteum. A layer of bone is thus formed, which serves as a starting-point for the formation of new bone by the proliferation of the osseous elements. The periosteum, moreover, not only does not enter into the production of callus after frac-

tures, but limits it, and in some cases actually prevents bony union, by its interposition between the fragments.

Proposition G holds that bone may be regenerated independently of the medulla, which may itself be reproduced. This is proved by the well-known fact that in certain instances of osteomyelitis, where the whole of the medulla and interior of a growing bone has been the seat of inflammation, producing a softened purulent pulp, enclosed in a shell of thin, though true bone, representing the cortical layer, the whole of the interior has been washed out, and after a prolonged period complete restoration of the calibre of the bone has resulted, though in some instances the growth in length has been interfered with, probably from involvement of the epiphyses.

Proposition H. The histogenetic phenomena support the foregoing observations, showing that the periosteum does not generate bone.

In conclusion, the author states "that a study of the whole subject, from histogenesis to experimental inquiry and pathological observation, shows that bone is produced and regenerated by proliferation of osteoblasts, and its development and reproduction can take place independently of the medulla and periosteum.

The practical import of these observations of Dr. Macewen is obvious. If the periosteum does not produce or regenerate bone, it is unnecessary to be so particular about its preservation in resections, and osteoplastic operations; whilst portions of loose bone in cases of compound fractures, trephining, and other operations where there is a loss of substance, should be carefully replaced, in the hope that they may serve as grafts, and result in reproduction of the lost parts.

THE VARIATIONS OF THE NORMAL KNEE-JERK.

SINCE Westphal announced its importance as a symptom in disease, this curious phenomenon has been the subject of many careful investigations. The recent papers of MITCHELL and LEWIS, in THE NEWS, on "The Reinforcement of the Knee-jerk," and of WARREN P. LOMBARD, in *The American Journal of the Medical Sciences* for January last, on the nature of the process, will be fresh in the memory of our readers. The latter gentleman has continued his researches, and in the first number of the new *Journal of Psychology*, issued from the Johns Hopkins University, has published a most thorough and exhaustive study of the variations of the normal knee-jerk, and their relations to the activity of the central nervous system.

The average knee-jerk varies throughout the day, being, as a rule, greatest in the morning, soon after breakfast, and being very much less at night. The extent of the jerk varies very much on different days. The cause of the variability is, to a large extent, the result of changes in the central nervous system. Fatigue, hunger, enervating weather, and sleep, all conditions, in fact, which lower the activity of the cerebro-spinal axis, diminish the knee-jerk, while rest, nourishment, invigorating weather, wakefulness, influences which stimulate the activity of the central nervous system, increase the jerk. For the details of the ingenious apparatus for estimating the vigor of the blow and degree of excursion of the foot we must refer to the original work, the great value of which consists in the fact that we have here, for the first time, a thorough study of the variations of the normal knee-jerk, from which, as a basis, we shall be in a better position to appreciate the differences met with in morbid states.

THE OIL OF TURPENTINE AS AN ANTISEPTIC.

THE present era in the history of medicine will, as some one has aptly expressed it, be known as the antiseptic or germicide epoch, and experimental therapeutists everywhere are employed in starting "cultures" and testing the anti-microbic power of the articles of our materia medica. As is usually the case, the call for an increased number of drugs to act in this manner, has been answered by the chemist and pharmacist with their accustomed promptitude, and at present we have laid before us a list of chemical combinations, purporting to be antiseptic or germicidal in their action, which has been evolved *pari passu* with the discovery of the microörganisms. Not satisfied with these, however, investigators go still further, and attempt to discover not only the drugs which are active but those whose effects are virtually negative. An example of this is well represented by the article of HOLMFELD in the *Fortschritte der Medicin* of October 1, 1887, in which he calls attention to the effect of the oil of turpentine on germs of various kinds. His method consisted in taking fresh colonies of the micrococcus prodigiosus and staphylococcus aureus. The vessels used were carefully sterilized, and agar-agar and gelatine were used as culture-beds. After some hours, oil of turpentine was applied, and its effects on the life and growth of the germs noted. Under these circumstances it was soon evident that the turpentine, unless applied in large amounts and for a considerable time, produced no effect of sufficient force to enable one to use the drug in ordinary everyday life for the purposes of antisepsis. Its irritant properties when concentrated, and its loss of the little retarding power which it possesses on microörganisms, as regards their growth, on dilution, exclude it from use on the tissues of the patient, and confine its sphere of value to the instruments employed. Certainly, from our present knowledge of the drug, we may rest assured that it is only proper to use it as an antiseptic when nothing better can be obtained.

SOCIETY PROCEEDINGS.

PHILADELPHIA NEUROLOGICAL SOCIETY.

Stated Meeting, October 24, 1887.

MORRIS J. LEWIS, M.D., IN THE CHAIR.

DR. F. X. DERCUM reported

A CASE OF UNILATERAL CONVULSIONS AND ANOTHER OF HEMIPLEGIA OCCURRING IN URÆMIA.

The following cases are of interest in connection with the observations of Raymond,[1] and of Chantemesse and Tenneson,[2] in the same field, and also in connection with two cases of hemichorea, associated with Bright's disease, recently reported by the writer.[3] They are as follows:

Case I.—H. S., colored, aged sixty-nine years, an inmate of the Philadelphia Almshouse, had, while in the out wards, occasionally suffered from convulsions. On several occasions he had been admitted into the epileptic ward of the hospital. Here it was noticed that he usually had several convulsions in close succession, each lasting about five minutes, and that after the attack was over he would fall asleep. His convulsions were always general, and attended with frothing at the mouth.

Mentally he was much impaired. He had delusions of persecution and sometimes imagined himself unable to walk. He was also at times violent and maniacal during the night, requiring to be strapped in bed.

On August 15, 1887, being much improved, he was, at his own request, again transferred to the out wards.

On August 27th, after having had occasional fits for several days, he was again admitted to the epileptic ward. On this day his convulsions seemed much worse than usual. They were almost continuous, and he was brought into the ward in a semi-conscious condition. In striking contrast with the seizures previously observed, the spasms now existed only on the left side, beginning apparently on the left side of the face, extending quickly to the left arm and thence to the left leg, being less severe in the last-mentioned member. During the quiescent periods it was observed that the left arm was paralyzed, as was also to some extent the left leg. The left side of the face was also paralyzed, the mouth was drawn toward the right, and the left cheek was flaccid. The conjunctiva of the right side was sensitive, but that of the left was not. The urine was drawn and examined for albumen, but none detected. The temperature was 97.2°; pulse 140. The sphincters were relaxed.

After the convulsions had ceased altogether the patient slept. Consciousness had evidently been absent during the attack, but on the following morning, August 28th, it had fully returned. The condition of left hemiplegia, however, persisted. His urine was now examined a second time, and revealed both albumen and casts, the latter in large numbers.

On the evening of the 28th the convulsions recurred, the patient having nineteen in rapid succession. On the morning of the 29th he was again conscious, and talked, but during the day he gradually became weaker and died at one o'clock.

An autopsy was held within the following twenty-four hours, and revealed the following: Calvarium of moderate thickness. Dura very adherent, the brain being removed in the calvarium. Pia not thickened, but its meshes very œdematous. Veins of vertex full. Pia presented milky opacities here and there, and was easily separated from the convolutions, a sub-pial space having been formed, which was distended with lymph; vessels of base extremely atheromatous. Brain as a whole soft, flattening by its own weight. Ventricles large and excessively pale. Choroid plexuses very pale and cystic. Velum interpositum presented whitish granulations. On section, the cortex was found pale and the white matter excessively œdematous. Serial sections of both hemispheres *failed absolutely* to reveal a *focal lesion.*

In the general post-mortem nothing of special importance was found except that the *kidneys were markedly granular* and fatty. The lungs were emphysematous, the heart fatty, and the aorta somewhat atheromatous.

Case II.—J. H., aged eighty-two years, white, in the out wards of the Philadelphia Hospital complained of weakness in the left arm and leg. This continued for four weeks, when he was admitted to the nervous ward of Dr. Mills, temporarily under my care. Here it was noticed that he dragged the left leg a little in walking and that he moved his left arm with difficulty. Sensation on the affected side was also dulled. The hemiparesis grew gradually more and more pronounced and was as sharply defined as a hemiplegia of organic origin, which, indeed, it was at first supposed to be. At last, though sluggish mentally, he was able to reply to questions and to make his wants known in his native language, namely German. Gradually, however, he failed in mind and body, incontinence of urine, delirium, and coma set in, and after having been in the ward two weeks he died. During life the urine had been examined and revealed a small amount of albumen.

On the day following death the autopsy was held and the following conditions noted. Calvarium and dura normal. Pia arachnoid loose and very œdematous, its meshes milky. Vessels of base very atheromatous. Ventricles excessively pale. Choroid plexuses presented numerous large cysts. Velum interpositum infiltrated. Brain very soft. On section, found to be œdematous throughout. Careful serial sections *revealed no focal lesion.*

The kidneys were found to be *much contracted and cystic*, markedly cirrhosed.

The other organs revealed nothing of consequence. The lungs presented some adhesions in the right pleura, the heart valves were thickened, the aorta was atheromatous.

Both of the above cases are doubtless to be relegated to affections of the nervous system occurring during and as a consequence of Bright's disease. Chantemesse and

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[1] Raymond: "Sur la pathogenie de certains accidents paralitiques observés chez des vieillards, heur rapports probables avec l'urémie," Révue de Médecine, Sept. 1885.

[2] Chantemesse et Tenneson: "De l'hemiplégie et de l'epilepsie partielle urémiques," Révue de Médecine, Nov. 1885.

[3] F. X. Dercum: "Two Cases of Hemichorea Associated with Bright's Disease," Journal of Nervous and Mental Diseases, xiv., August, 1887.

Tenneson,[1] have already recorded two instances of unilateral epilepsy occurring in the course of this affection and have verified one of them by post-mortem examination. Their cases are in every way a counterpart of the case here reported. The convulsions were strictly limited to one side, and the autopsy revealed merely general œdema of the brain and chronic interstitial nephritis.

The objection may be made that these phenomena of hemiplegia, hemichorea,[2] and hemi-eclampsia, are due simply to atheroma of the arteries of the brain, by means of which a portion of the cortex or a tract of the internal capsule may receive an imperfect blood-supply, be shut off, as it were, from its necessary amount, an oligæmia and œdema being the result. The weight of evidence, however, is against this interpretation. There are absolutely no areas of local softening or local necrosis to be detected, and the œdema is not confined to a spot but is general, is evenly diffused, and apparently as much in one hemisphere as in the other. Besides, the ingenious experiments of Raymond[3] leave no doubt as to the possible one-sided action of the nervous system in uræmia. It will be remembered that he first mutilated the sympathetic system of an animal by removing the superior cervical ganglion of one side. In this way he threw the entire nervous apparatus, so to speak, out of balance. He then ligated the hili of both kidneys, and the convulsions that ensued were limited to one side. He repeated the experiment a second time with the same result. It seems as though the two halves of the nervous system bear unequal powers of resistance and that the weakest yields.

DR. OSLER asked if, at the autopsy, special attention was paid to comparing the two sides of the brain. It was some time since he read the articles to which reference had been made, but his impression was that more œdema had been found on the side of the brain opposite to the side on which the paralysis was present. An interesting point in these cases is that it appears to be absolutely impossible in certain cases to make a satisfactory diagnosis between cases of hemiplegia from apoplexy and cases of hemiplegia from uræmia. He recently went over this question with his class in the case of a man brought into the hospital with hemiplegia, unconscious, and with albumen and tube-casts in the urine. He said that it was in all probability a case of hemorrhage, but that it was impossible to exclude positively hemiplegia due to uræmia. He said that it might be a case of œdema of the brain from uræmia, in which the œdema, being more marked on one side of the brain, or from some other cause, the paralysis was unilateral. He had seen one case of unilateral convulsions occurring in a man who was brought into the General Hospital at Montreal some eight or nine years ago. He had convulsions for three or four days. At the autopsy in that case nothing was found in the brain that would account for the convulsions. At that time we were not so familiar with œdema of the brain, or with the relation af œdema of the brain in Bright's disease as we are now.

DR. JAMES HENDRIE LLOYD said that Dr. Dercum may recall the fact that, at the meeting of the American Neurological Society two years ago, he referred to a case similar to those reported. The patient was a boy, aged seventeen years. When he saw him he was in a comatose condition with convulsions, which were confined to the right side of the body. The boy had been a chronic sot, and died a few hours after he saw him. At the autopsy there was nothing found in the brain to account for the condition. The only lesion was the "pig-back" kidney. This case was undoubtedly one in which the symptoms resulted from uræmia. He thought that in the intervals between the convulsions it was possible to demonstrate the fact that the side on which the convulsions occurred was paralyzed.

DR. MORRIS J. LEWIS said that a man, aged twenty years, a heavy drinker, under his care, had albumen with granular and oily casts in his urine. He suffered with frequent convulsions, which usually followed the periods of heaviest drinking; and on two occasions the convulsions were most marked on the right side, and were followed by paresis, which lasted two or three days. The young man was placed in a position where he could not obtain alcohol, and has apparently made a complete recovery. There is at present no albumen, no casts, and no paresis.

DR. DERCUM said that, to the best of his recollection, the authors to whom he had referred, have noted no local œdema. He had made three post-mortems in cases of well-marked hemi-disease occurring during uræmia, and he had found no more œdema on one side than on the other. The cases reported by Drs. Lloyd and Lewis are of special interest as occurring in young persons. Most of the cases reported have occurred in old people. With reference to local cerebral manifestations from a general cause like uræmia, he recalled the case of a gentleman under his care for several years who suffers from chronic contracted kidney. He now and then has general uræmic convulsions. Some years ago he had the following remarkable seizure. He went down town to attend to some business, and while in a bank he suddenly became aphasic. He was perfectly conscious. The aphasia continued for several hours. This looks like another instance of a local manifestation from a general cause.

MENTAL AFFECTIONS ASSOCIATED WITH CHRONIC BRIGHT'S DISEASE.

DR. WILLIAM OSLER said that as it was in close connection with the paper of Dr. Dercum, he should like to make some reference to the occurrence of certain mental affections which come on in connection with chronic Bright's disease. It is well known that certain mental phenomena occur in connection with chronic renal diseases besides simple uræmic coma. He had reported one case of violent mania in a man aged forty-two years, the subject of Bright's disease. When brought to the hospital, he had been maniacal for three or four days. He subsequently became comatose and died. A very interesting case was recently under his care in the University Hospital. A man was brought to the hospital Thursday evening. He saw him on Saturday. He was then quiet, in a semi-dosing condition, but could be aroused and gave a very intelligent account of himself. The whole clinical picture was that of chronic interstitial nephritis. He thought it not improbable that the man might pass into a condition of

[1] Loc. cit. [2] Dercum, loc. cit. [3] Loc. cit.

coma. There was nothing to attract special attention to his mental condition, and he did not regard his condition as critical. That night he got out of bed in the absence of the attendant, wandered about the ward and finally jumped out of the window. It was subsequently learned that, before admission to the hospital, he had been violent, requiring two or three men to hold him. We were not told this when he was brought to the hospital. He had no doubt that this was an instance of mental disturbance due to chronic nephritis. He was told by one of the physicians who had attended him that the man was full of delusions. He thought that his wife and others were persecuting him.

He saw another interesting case a year ago last Christmas. This occurred in the practice of Dr. Mullin, of Hamilton, Canada. Here there was also a medico-legal question. It was whether or not the man was in a condition to make a will. There was no doubt as to the existence of chronic Bright's disease. The mental condition was peculiar. He believed that his wife and others had designs upon his life, and it was with difficulty that he could be persuaded to take food. He thought that people were persecuting him. Although he gave a very intelligent account of himself, it was not considered advisable that he should make his will at that time. He was placed upon a somewhat more active treatment than he had previously received. This man subsequently did well, his mind had cleared, and he recovered sufficiently to get about and to make his will.

DR. DERCUM said that the negro who was the subject of unilateral convulsions, had also presented, at various times, periods of maniacal excitement and had also had delusions of persecution.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Special Meeting, September 17, 1887.

THE PRESIDENT, THOMAS M. DRYSDALE, M.D., IN THE CHAIR.

REMARKS ON THE REMOVAL OF UTERINE FIBROIDS BY ABDOMINAL SECTION.

DR. GEORGE GRANVILLE BANTOCK, of London, began by saying that the tumor which formed the text for his remarks had been removed the day before, from a single woman aged thirty-six, a patient of Dr. Joseph Price, and weighed between five and six pounds. At first sight he had doubted the propriety of operating, because there seemed to be no indication for interference beyond the rate of growth of the tumor, and because the patient, as he had been assured by Dr. Price, very much minimized the extent of her sufferings. The matter was thoroughly explained to her; the danger of the operation was made clear, and it was pointed out, that if operation were necessary at some time, she would never be in a more favorable condition for it. She decided in the affirmative, which was fortunate, for it would be seen that the tumor had in its upper part already undergone cystiform degeneration, an incision into the cyst, giving exit to over half a pint of a thin sero-sanguinolent fluid. He was happy to say that the condition of the patient was most satisfactory.

Dr. Bantock then proceeded to the general subject of the removal of uterine fibroids by abdominal section, which, he said, was a very important one. He pointed out that operators were to be divided into two schools: the one preferring to treat the pedicle by the intra-peritoneal method, the other preferring the extra-peritoneal. The advocates of the former based their arguments on the success which has resulted in the operation of ovariotomy, from the introduction of the intra-peritoneal ligature of the pedicle. But it was scarcely necessary for him to show how fallacious such reasoning was, for the conditions were so dissimilar. It was true that there were many different forms of ovarian pedicle: it might be large or small, long or short, broad or narrow, thick or thin, but they were all essentially of the same character and capable of being more or less easily secured by ligature which might be confidently relied on if properly applied. On the other hand, the uterine stump is very different. It is composed of muscular fibre, white and yellow elastic tissue, forming a structure so pecular that under the pressure of a ligature tied with as great a strain as it will bear, it yields before the compressing force to such an extent that in a few hours the ligature may be quite loose. This occurs even when the pressure is exerted with the aid of a screw, and he pointed out to those who were present at the operation that at the first tightening of the screw there was complete arrest of the bleeding for the time, but that at intervals of ten or fifteen minutes the screw would take a turn or more with facility. In this way an amount of constriction was attainable which, if attempted at once, would probably lead to the breaking of the wire or the cutting of the tissues. He had just come from the meeting of the American Gynecological Society, where he had read a paper on the subject of the treatment of the pedicle, and where the opposite school was so well represented in the person of Dr. Martin of Berlin, and he had the satisfaction of feeling that, for the time at least, he had established his position.

Now the pedicle in this operation varies very much, not in the nature of the tissues forming it, but in the extent and manner in which the parts are involved. Thus the tumor may spring by a distinct pedicle, from the fundus, or some part of the free surface of the uterus. In such a case the application of the extra-peritoneal method is very simple, and his results have been uniformly good; for in thirteen of these cases, all the patients recovered. On the other hand, of all the cases of this kind treated by the ligature and intra-peritoneal method, five in number, only one was successful. Moreover, he had attempted the method in several cases and had been obliged to resort to the extra-peritoneal method after failure of the ligature, though preceded by compression with very powerful forceps. When the body of the uterus is extensively involved in the growth then matters may be very much complicated. The broad and ovarian ligaments may be so lax that it is a simple matter to include ovaries and uterus in one encircling loop. Such a case is little more difficult than an ovariotomy done with the clamp. But the ovarian ligament may be so short on one side that it has to be ligatured separately. That had to be done in the case that furnished the occasion for these remarks. As a rule, it is advisable in such a case to secure the uterus and other ovary first, to transfix the stump with the supporting pins placed across the abdominal wound, to cut away the tumor, trim the stump, and then remove the ovary, lest in tightening the

instrument any part of the ovarian pedicle should be pulled through the loop of the ligature. In the case under consideration it was not necessary to observe this order as the broad ligament on the left side was very lax while the ovarian ligament was very short. On the right side the ovary could be raised quite out of the pelvis. Sometimes both ovaries are thus tied down and have to be secured separately. The same precautions must be observed.

But the disease may involve the whole of the body of the uterus, properly so called. Then one must make a pedicle, and this is the most difficult state of things to overcome. It was for such a state of things he had devised his method of partially enucleating the body of the uterus and the lower portion of the tumor. The large vessels of the ovarian plexus are secured on each side by two pairs of forceps or stout ligatures, the lower of each pair being placed an inch higher than the level of the proposed constriction. Then the peritoneal investment is divided all round on a level between the upper and lower forceps, and reflected downward toward the cervix and nearly to the level at which the loop of the serre-nœud is to be applied. In doing this it is necessary to seize the peritoneal edges with forceps placed at intervals of about an inch. When the reflection is completed these instruments are collected and raised up and the wire loop is applied outside of the peritoneum. Sometimes, and especially when it is desirable that the patient should not lose much blood, it is well to throw an elastic ligature around the uterus and broad ligaments as soon as the division of the peritoneum is effected. Applied in this way, it serves to draw in the broad ligaments toward the uterus and to enucleate the tumor. It is to be removed as soon as the serre-nœud is applied above or outside it, of course. In this way an inch or more in the length of the pedicle may be gained. It should be remembered, that in this method it is necessary to have the stump so long that the whole of the constricted portion must be outside the peritoneal cavity; in which position it is to be retained by the transfixing pins, which pass through the stump just outside the wire loop and rest on the parietes on each side of the wound. While the peritoneum is being reflected, the tumor should be held up by the assistant, and thus it rises gradually out of the pelvis with the uterus. One precaution to be observed very carefully in these cases, is to transfix with the pins before cutting away the tumors. He once lost a patient through the non-observance of this precaution. The last steps are to cut away the uterine tissue to within a quarter of an inch of the pins, and to trim the loose peritoneum by cutting it away to within half an inch of the loop and then stitching it from the opposite side across the surface of the stump, to hold it all well together. Before closing the wound give the screw a final turn if it will allow of it. Remember that the distal portion of the stump must slough off, that it is desirable that the amount of this tissue should be as small as possible, and that it is necessary to keep it out of the peritoneal cavity.

You may have heard great stress being laid upon the character of the mucus in the uterus and cervical canal, and you may have observed great care used in wiping it away and setting aside the sponge employed for the purpose. On operator has gone so far as to pass an iodoform pessary down the canal for the purpose of disinfecting. This is a practice with which he has no sympathy; nor is it rational or founded on any sound reasoning. He looked on it as a bugbear, and regarded it as a means of withdrawing attention from other and more important matters of detail. As to comparative results, Martin had just reported 84 cases with a mortality of 25; while Dr. Bantock was able to point to 72 cases with only 12 deaths.

There is some misapprehension of the value of terms. Some operations are termed aseptic, but the result only can prove any operation to have been aseptic. All operations are or are not performed upon an antiseptic method. Dr. Bantock has now discarded all antiseptic agents. When Listerism was first introduced, he used it in all its details, and continued to do so until he lost a patient, and had others affected by what he felt sure was carbolic poisoning. An easy and sure test for this is the absence of sulphates from the urine. In one case of carbolic poisoning the temperature rose to 107° and the patient was almost moribund; by means of wet sheets and ice-packing the temperature was brought down to normal in eight hours, and she recovered.

In one fatal case the kidneys were found congested; there had been acute suppression of urine. After this experience he gradually reduced the strength of the carbolic washing fluids to one per cent. Coincidently the proportion of sulphates in the urine increased and there was an absence of high temperature and other symptoms of hitherto unknown origin. Now as 1 to 40 is the weakest solution that can be useful as a germicide, a one per cent. solution could serve no good purpose, and the carbolic acid was omitted altogether. Afterward he tried doing without the spray, and results steadily improved. Of his last one hundred ovariotomies only three have been lost, and of the last seventy-eight only one was lost. But ovarian statistics do not apply to uterine operations.

DR. W. H. PARISH was present at the operation yesterday morning, and it was a complete illustration of what is necessary in all operations. First, in saving blood; Dr. Bantock was very careful to use hemostatic forceps to secure every bleeding point in the abdominal wound before opening the peritoneum. The same economy of blood was observable in every step of the operation. Great care was also taken to avoid shock from cooling the intestines by the contact of air and consequent evaporation. Large, flat sponges were pressed in to cover the intestines and prevent their escape or the entrance of blood or discharges into the peritoneal cavity. He is extremely careful about the cleansing of his sponges before reintroducing them into the wound. In placing sutures, he first passed several silkworm-gut stitches close to one another near the stump. After the wound was entirely closed it was covered with clean absorbent gauze—a pile of this was placed over the stump. The whole was secured with a cotton binder, using no adhesive plaster. So, while using no germicide at any time, every care was taken to make the operation an aseptic one. Would Dr. Bantock consider it safe to transfix, ligate, and drop the pedicle in case of a uterine fibroid having a thin pedicle as long as a finger? Dr. Parish had done so on one occasion and the patient died.

DR. BANTOCK replied that a uterine fibroid with a finger-like pedicle, he had never seen. He has observed

none smaller than half his wrist. He thinks the extra-peritoneal method safer in every case.

DR. LONGAKER made some remarks about securing the peritoneum over the base of the stump, and asked what kind of suture Dr. Bantock preferred for that purpose.

DR. BANTOCK said that any kind of suture would do, it was merely to secure a neat stump that would not spread out over the wound. He prefers fine silkworm-gut for sutures. He introduces his needle from within, close to the edge of the peritoneum, and makes sure of a hold in muscle or aponeurosis, and then through the subcutaneous fat and skin to secure a firm hold and prevent future hernia.

Stated Meeting, October 6, 1887.

THE PRESIDENT, THOMAS M. DRYSDALE, M.D., IN THE CHAIR.

DR. M. PRICE reported a case of

ADHERENT INTESTINES FROM PERITONITIS, SIMULATING FIBROID TUMOR.

Mary J., colored, aged seventeen years, received a kick in the belly from a horse, about eighteen months ago. She was confined to her bed for a number of weeks, and was a confirmed invalid up to the time of operation, suffering great pain in the lower part of the abdomen, the region of the injury. She was unable to work or do anything toward her own maintenance. Upon examination, a hard mass was found high up and to the right of the uterus. Owing to her condition and the repeated efforts for her relief, it was decided to open her and see if the offending cause could be removed. This was done June 17, 1887, Drs. C. Penrose and J. Price, assisting, and Dr. W. Welsh present. The bowels were found completely matted together with an ill defined mass at the right side. Nothing was found that could be removed. The omentum could not be found. Dr. J. Price suggested the separation of all the old adhesions of the bowels, peritoneum, and mesentery. These adhesions involved the entire length of the small intestine; binding the folds of the mesentery and knuckles of intestine in one solid mass. The adhesions were of such strength that many of them had to be cut through with scissors. The scissors, hand, and sponge were the instruments used in the separation of the adhesions. This method was followed until every knuckle of intestine and fold of peritoneum and omentum was separated. The bowel was almost completely occluded by the bands. Irrigation with distilled water at a temperature of 110° completed the operation. After this operation was finished, the indurated mass could not be found. She reacted well, and was freely purged with epsom salts. She made an uninterrupted recovery, and said she had less pain the day after operation than at any time after the kick.

She is now perfectly well and able to be about her work. As a distinct surgical procedure, Dr. Price knows of no similar case on record. He opened a man a year ago for great pain in the region of the gall-bladder, and tore loose the adherent viscera from the abdominal wall so as to complete his examination, but finding nothing wrong, closed up the incision; the pain was cured! but this operation was not performed with the same object in view as in the first case. Dr. Duncan in his book on *Diseases of Women*, page 49, gives a case in point. "I remember a case diagnosed as a fibrous tumor of the uterus, a hard rounded mass as big as a child's head, above the brim of the pelvis, very slightly tender, and fixing the uterus. The young lady died, and at the post-mortem examination it was found that there was no fibrous tumor at all. It was adhesive perimetritis, a pocket of coherent intestines, which formed a hard mass and had led to the deception of eminent and experienced gynecologists."

SPINDLE-CELLED SARCOMA OF THE OVARY.

DR. M. PRICE also reported the case of Mrs. M., aged forty-four years. Married twenty years, sterile; always regular; had always enjoyed good health up to January 1, 1887, when she was greatly troubled with inability to hold her water and with constant pelvic pain, with a feeling of fulness, and an entire suppression of the menstrual flow, which had been regular up to that date. The incontinence of urine was relieved about the middle of February by the escape of the tumor into the abdominal cavity. The tumor grew rapidly, and when it had reached some two inches above the umbilicus, the patient thought she might be pregnant, and sought advice as to her condition. Upon examination I found the uterus small, the left ovary and tube healthy and posterior to the tumor. The right ovary could not be found, but in its position the pedicle of the tumor could be mapped out. The removal of the tumor was advised. Operation June 11, 1887, with assistance of Drs. J. Price, Burns, and Penrose. A spindle-celled sarcoma of the ovary, weighing five pounds, was removed. The patient reacted well. On the evening of the second day there was a slight rise of temperature and quickening of the pulse. Fearing peritonitis, I ordered a tablespoonful of epsom salts every two hours until the bowels were freely moved. Six doses were given; the next day the pains had disappeared and temperature and pulse were normal. She has made a perfect recovery.

DR. DRYSDALE remarked that the successful result of the operation in this case was a proof of its justifiability, but he thought that, generally speaking, it would be of doubtful advantage to perform abdominal section for the relief of the consequences of peritonitis, as adhesions would be likely to reform immediately after the operation.

DR. PRICE explained that the active movements excited by the epsom salts prevented new adhesions from forming. He made some remarks on ovarian tumors, and stated that although seeming to be of fibroid character and called fibroid at time of removal, microscopic examination had, in his experience, uniformly shown them to be spindle-celled sarcomata.

DR. PARISH remarked that it is always allowable to break up inflammatory adhesions in abdominal operations. The new adhesions formed are not firm.

DR. H. A. KELLY said that a year ago he had examined a small tumor, about the size of a pea, distinctly arising from the ovary, and found it to be a fibroid; it was quite hard. He had, three months ago, removed from a girl of eleven years, a large sarcoma of the ovary; she recovered. The intestines might be bound together into a mass by plastic lymph, and if there was no unnatural position of any portion, no kink nor strangulation, no symptoms would be developed. The patient would feel no pain and would be conscious of nothing

wrong. In an operation all bands and local adhesions should be broken up and kinks or knuckles should be liberated.

DR. J. PRICE said that trouble will sometimes follow breaking up of old adhesions in the removal of cystomata, local pain and alarming local and general symptoms. He thinks the matting together of the intestines resulting from general peritonitis a cause of death in some few cases, and could be averted by prompt surgical interference. He described a case which he had seen recently as consultant, and in which he advised immediate operation. The attending physician would not consent until he thought his patient dying from peritonitis. The abdomen was now opened and the pus and lymph removed by irrigation and sponging; all adhesions were freed, and the patient commenced to improve immediately and is to-day a well woman. The operation was the only way in which her life could be saved. In his brother's case the intestines were freed by free dissection, pressure was applied afterward to hold the intestines back in the loins and keep them immobilized. The hemorrhage during these operations was very slight. Dr. J. Hoffmann had, in a recent operation in which Dr. Price assisted him, found fourteen inches of the large intestine adherent in an operation for the relief of an umbilical hernia, requiring a long and masterly dissection. If such adhesions are not liberated, the operation of dividing the stricture and simply liberating the strangulated portion may be a failure in saving life. Here, again, Mr. Tait, in his deep surgical wisdom recognizes the importance of opening the abdomen for strangulated hernia in the median line, that inflammatory adhesions and strictures may be liberated in the most scientific manner. Adhesions in pelvic inflammations are very common, and in operations they should always be released. Hard bodies are frequently discovered in the tube and ovaries. Their appearance suggests fibroid growths, but they are in reality sarcomatous. The round ligaments alone do not seem liable to disease.

DR. MONTGOMERY was very much interested in the subject of this paper. The method of first breaking up the adhesions and then preventing their reproduction by peristaltic motion excited by saline purgatives is similar in principle to the treatment of ankylosis in joints; breaking up adhesions and passive motion subsequently. Doubtless all present can bear witness to the beneficial action of saline laxatives in dysentery and other inflammatory intestinal affections. He had recently assisted Dr. Warder in the removal of an abdominal growth. The diagnosis had been uterine fibroid, but when the abdomen was opened the tumor was found to be of ovarian origin. It was very large and hard. Dr. Daland had made an examination of it and reported it to be fibroid in character. There was considerable ascitic fluid.

DR. LONGAKER made some remarks on the clinical history of adhesions. Some months ago, the specimens from a case of obstruction of the bowels ending in death were shown by him at the Pathological Society, This was a case of encysted, retro-uterine hæmatocele, the appendages on both sides being involved in the mass of adhesions, from which mass there passed a band to the lower portion of the descending colon about nine inches above the sigmoid flexure, where it was so attached as to form an acute flexure. At this point the bowel was in a gangrenous state and the colon above was greatly distended while the portion below the obstruction had a lumen just admitting the passage of the little finger. The patient manifested no symptom of difficulty until four or five days after labor, when some nausea and vomiting occurred, The points of interest are, that while the autopsy demonstrated the feasibility of an operation, the symptoms were at no time sufficiently definite to warrant a diagnosis. The patient must have had the trouble a long time without it giving rise to any but vague and indefinite symptoms. As involution of the uterus took place this knuckle of bowel was slowly dragged down into the pelvis.

DR. M. PRICE, in closing, said that the appearance of his first described patient was remarkable. The long-continued suffering from peritonitis had so wasted her that she looked as if she had passed through a wringing-machine; her chest and belly were flattened out. If no symptoms resulted from the adhesions, there would be no call to operate for their relief. But in all cases the intestines have sharp bends and curves, and form knuckles, which are caught under lymph bands, which tie the intestines together and contract them, and sooner or later symptoms of obstruction must be developed. He does not believe in antiseptic surgery as applied to the abdomen and the peritoneum. If he did believe in microbes, tadpoles, and the other insects so much talked about, he would be afraid to operate, for their presence, if all that is said about them were true, would do more harm than the peritonitis, and more than the operation would do good.

DR. H. A. KELLY considered this a most important subject, and one on which we are greatly in need of enlightenment. His own experience had taught him several interesting facts in regard to peritoneal adhesions. Bands of adhesion, or the adhesion of a small area of intestine in such a way as to make it liable to kink, doubling it sharply on itself, are exceedingly dangerous, and should never be allowed to remain. But there is a class of adhesions by no means dangerous, not productive of symptoms, and which should not be interfered with. We must bear in mind that the adhesion of opposing serous surfaces is a conservative process preventing the continuous pouring out of effusion, and, in event of tuberculous peritonitis, probably starving out the disease by taking away its pabulum. The least dangerous variety of adhesion is the general adhesion of the gut *in situ*. He operated two years ago on a case in which all the intestine adhered, looking like one great bag when the peritoneum was opened. It was a case of tubercular peritonitis tending to a cure in this way. He simply dried out the peritoneum, and besprinkled it with iodoform to check secretion, and the patient, who has been constantly under his eye ever since, has not had any trouble from her adhesions. Also those cases of adhesions of lesser intestines in the pelvis, in which they lie adherent in normal relative position, in full round curves without kink, do not, so far as he has been able to observe in a large number of cases, ever do harm.

DR. H. A. KELLY read a paper on

PALPATION AND SOUNDING OF THE FEMALE URETERS,

in which he stated that he made it his routine practice

to examine all gynecological cases with reference to the condition of the ureters. Since Dr. Sänger had demonstrated to him, in the summer of 1886, the facility with which both ureters could be outlined through the antero-vaginal wall, and traced back to the posterior pelvic wall, he had made a long series of confirmatory observations, and, in some instances, demonstrated the value of such observation by finding disease of the ureter. He had added to this a method of his own, by which the ureter is picked up and handled bimanually, and with the exact knowledge thus gained as to its position, he then proceeds to catheterize it with Pawlik's catheter. He detailed a case in which the catheter was thus introduced free-hand into the ureter, and passed back to the posterior pelvic wall. The urine escaped in little gushes of from ten to fifteen drops every ten or fifteen seconds, evidently accumulating beyond the catheter until the pressure became sufficient to dilate the ureter and let the urine pass down into the eye of the instrument. About three drachms of urine were drawn in this way, and, on removing the catheter, about five ounces of urine were drawn from the bladder. A disadvantage of Pawlik's catheter is that the sharp eye sometimes catches the mucous membrane of the ureter.

Dr. Kelly showed some sounds which he had had constructed some time since, when studying the subject, but considered them inferior to Pawlik's instrument. The value of knowledge which may be thus gained was illustrated by reference to a recent case of severe operation, removing adherent, pus-containing ovaries and tubes. On the second day urine appeared in the tube, and flowed freely. The only way of determining from which kidney this flow came was by catheterization of the ureter.

DR. CHARLES M. WILSON thought that such an instrument used in cases with a history of pyelitis might be the cause of serious trouble. He has tried in a number of cases to palpate the ureters by Dr. Kelly's method; in a few elderly women he has succeeded, but in nulliparæ with firm tissues he could not distinguish the ureters.

DR. J. PRICE considered these methods of examining the ureters of but little practical use, for trouble does not exist in the ureters alone, but in all the pelvic organs and kidneys and when these are distorted by new growths and inflammatory products it would be very difficult to locate the ureters. The case cited by Dr. Kelly in his own experience shows how difficult it is even with the fingers in the pelvis while removing diseased appendages, to locate the ureters through one layer of peritoneum, to say nothing of the conditions when we have to deal with inflamed and thickened vaginal walls.

DR. MONTGOMERY thought it would require extreme skill to pass such fine instruments without doing injury to the mucous membrane. Dr. Bozeman, in a paper read before the Ninth International Medical Congress recommended dilatation of the ureters and washing out of the pelves of the kidneys and ureters in pyelitis; even advising splitting of the bladder through the vagina in order to reach the orifices of the ureters.

DR. WALKER was very much interested in this subject and has been desirous of accomplishing this procedure in some patients that he had in his care in the Philadelphia Hospital. He had succeeded in palpating the ureters in males but not in females. This ability to pass a catheter into the ureter will enable the surgeon to determine in any given case which kidney is diseased.

DR. KELLY, in closing, said that there is no danger in the exploration; the instrument must be lightly poised between thumb and forefinger and gently passed in, or not at all. The sensation when the end of the catheter is caught by the ureter and it begins to pass up the tube is characteristic. Dr. Walker is certainly correct in his high estimate of the great value of the possibility of sounding the ureters free-hand in certain puzzling renal cases. As to Dr. Price's remark, cases in which there is much pelvic cellulitis are not those in which we desire to sound the ureters. Ureteritis is a distinct, well defined disease. He treated one case which he cured by opening the base of the bladder. The whole trouble lay in the extremities of the ureters. The first to suggest splitting the vesico-vaginal septum and rolling the ureteral orifices and thus under guidance of the eye catheterizing them, was Dr. Emmet. Pawlik's work was but the culmination of a vast number of attempts to accomplish this, from Suchmanns, who attempted to clamp the orifices of the ureters, and Silbermann, who tried to choke them by a bag of quicksilver, and Simon's dilatation of the urethra, and Neumann and Grünfeld, who tried to reach it by endoscopic methods. Sänger showed how readily the ureters can be palpated, and Dr. Kelly thinks that by combining a careful palpation according to his own method and following this with catheterization of the ureters already so distinctly located, he has added a method of real value, simplifying this free-hand difficult subject. Great skill is not required thus to palpate the ureters.

DR. HIRST reported a case of

TYPHOID FEVER IN A PREGNANT WOMAN.

The impression still prevails in some quarters among general practitioners, that typhoid fever will not attack a pregnant woman; an idea originating doubtless in the teaching of the famous Rokitansky, who believed that the condition of pregnancy granted a woman immunity from this disease. It is unnecessary, of course, to point out the fallacy of this opinion or to refer to the statistics of 322 cases of typhoid fever during pregnancy collected by Charpentier. The case now reported occurred in a young primigravida, who was admitted to the Philadelphia Hospital in the second week of the attack. The disease presented all the characteristic symptoms in a typical manner, leaving no doubt as to the diagnosis. On the tenth day after her admission she gave birth to a child which corresponded in its development to the seventh month of intrauterine life. The infant died two weeks after birth, but, unfortunately, the cause of death was not ascertained. One interesting feature in this case was the effect of labor upon the temperature. As uterine contractions began the woman's temperature was 104.2°, but it steadily lowered as labor advanced, until directly after the expulsion of the child it was only 95°. Under the influence of external heat and stimulants hypodermatically, the patient reacted and made a good recovery.

DR. LONGAKER remarked that his experience taught him that high temperature in typhoid fever in the pregnant woman was incompatible with fœtal life. He had

been recently associated with such a case which ended fatally.

DR. CHARLES M. WILSON had under his care a pregnant woman with typhoid fever in whom the temperature rose as high as 106° and was not reducible for several hours, but the child survived and was delivered alive at term. Numerous similar cases are on record, so a temperature as high as 106° is not necessarily fatal to fœtal life.

DR. MONTGOMERY remarked that in the majority of cases maternal typhoid fever is fatal to fœtal life. Premature labor has occurred in all such cases that have come under his notice. In one such case the temperature rose to 105°; miscarriage occurred, and was followed by a fall of temperature to 97° with collapse, from which it was very difficult to restore her. The temperature again rose to 105°, and after a protracted illness the patient recovered.

DR. W. S. STEWART remarked that in one case of typhoid fever, about the sixth month of pregnancy, in which the patient became emaciated to a remarkable degree, she recovered and was delivered of a living child at term. The temperature did not rise very high.

DR. J. C. DA COSTA stated that these cases do not all abort or die. He recalled one case in particular, with a temperature of 105½°, with characteristic typhoid eruption and intestinal hemorrhage; four stools containing, each, from one to two pints of blood having been passed, which went on to full term, twins being delivered alive.

DR. HIRST, in closing, stated that although the temperature of his patient remained at 95° for over twelve hours, despite all that could be done to raise it, she did not appear in collapse, but looked and felt quite comfortable. The prevalent idea as to the danger to the fœtus of high temperature in the mother, would appear to be erroneous. Runge's well-known experiments are often quoted to support the view that the fœtus is in danger if the mother's temperature reaches 104°; but Doléris, in 1883, showed that if the maternal temperature is gradually and not quickly raised, the fœtus remains unaffected. Runge, in a second set of experiments, found that Doléris's conclusions were correct. Preyer, on one occasion, actually found a temperature of 110° in the anus of a living fetus.

DR. PARISH considered that the high temperature in disease was accompanied by blood changes which contributed to the fatal result.

DR. J. M. BALDY read a paper on the administration of

SALINES IN PERITONITIS FOLLOWING ABDOMINAL SECTION.

Twenty-five per cent. of the deaths following abdominal section are attributable to peritonitis (Agnew). Taking the hint from Mr. Tait, he never takes any steps to prevent a movement of the bowels after operation, and routinely administers salines on the second or third day, repeated in small doses, especially when there are distention and persistent bilious vomiting. The results following this course have been most satisfactory. He had used salines in large concentrated doses (one ounce), repeated several times and aided by large (two quarts) turpentine enemata in fully developed peritonitis, with best results. The symptoms begin to subside almost immediately when the bowels commence to discharge watery stools. The treatment is in every way preferable to the use of opium, and is logically infinitely better than the narcotic. Opium only does good by relieving pain, at times, in enormous doses. The bowels are already "in splints" from the disease and do not need the aid of the drug for that purpose. Opium, however, does a world of harm. It helps keep the bowels in splints and so favors the formation of those great masses of adherent intestine and organized bands which we find so often the cause of subsequent intestinal obstruction. Still worse, it closes all the avenues of escape for the poisonous products of inflammation found in this great lymph sac, and in this manner keeps up the inflammation much more surely than the rubbing together of the peritoneal surfaces will. With salines, however, an active peristaltic movement is kept up, which tends to prevent the formation of adhesions and bands. They drain the abdominal cavity of the products of inflammation, and by depleting the blood-vessels of the intestines and peritoneum tend to prevent the throwing out and organizing of lymph to any great extent. There are, of course, dangers in the production of large, watery stools, but the treatment must be used with care and judgment. He would suggest that in the after-treatment of abdominal section the bowels be moved as a matter of routine on the second or third day after operation. That on the first access of pain and soreness with distention, a brisk purgative be given at once. In case a general peritonitis is developed, the production of large watery stools should be secured as quickly as possible, by purgation and stimulant enemata; the amount of purgation to be controlled in accordance with the strength of the patient. The salines, as suggested originally by Mr. Tait, are perhaps the best of the purgatives for the purpose.

DR. WATSON remarked that Mr. Tait had been led to the use of salines because he had always observed bloody serum in the peritoneal cavity after death from peritonitis. Dr. Watson has been in the habit of using saline laxatives whenever he has found signs of beginning peritonitis after labor. Once after ovariotomy, and again after a very difficult labor he found their use of very decided benefit in cutting short threatened attacks of peritonitis; both cases had made good recoveries.

DR. LONGAKER remarked that Dr. Bantock, while in this country recently, expressed entire want of faith in any benefit to be gained from the use of salines in peritonitis.

DR. PARISH explained that peritonitis as seen by Dr. Bantock was always of septic origin, and the patients would die under any treatment. That form of peritonitis is preventible. The saline treatment is quite reasonable; it is an adaptation of an old method of treating dysentery and inflammatory diseases of the intestines.

DR. WALKER had employed salines as depleting agents in various inflammatory diseases. When the depletory action is desired, concentrated solutions should be administered. A weak solution will act simply as a laxative.

DR. CLEEMANN had had success and failure with both the opium and saline methods of treating peritonitis Salines had been recommended years ago by Dr Meigs in the treatment of peritonitis. Several suc-

cessive attacks of typhlitis in one patient had been subdued under the use of opium. A case of peritonitis under his care had shown progressive relief after every movement of the bowels, the result of a dose of castor oil. As the opium treatment has been adopted by the majority of physicians, it is presumably the best.

DR. KELLY believed that serious consequences will often be avoided by a timely free depletory purgation. He had used calomel and soda, but now considered salines better. All peritonitis is not, however, as the speaker would urge, to be so treated. There is a dry form, which he has verified by post-mortem examination, which corresponds to the friction dry stage of pleurisy, which is better treated by opium. We must bear in mind that the depletion of the intestinal villi only quickens the absorptive activity of the peritoneum, and a large accumulation within the peritoneum may really be too great to be thus disposed of. Here the drainage tube saves lives by disposing of the excessive and often unavoidable serous weeping from wounded peritoneal surfaces. He calls this his *fifth emunctory*, and it is better than all the natural emunctories in these cases, when properly handled, disposing of accumulations at once, without in the least taxing the powers of the patient. Next to the drainage tube, the best emunctory is that nearest the field of activity, the intestinal tract, and, next, but much inferior to this, come the kidneys, then the skin, and, lastly, the lungs.

DR. MONTGOMERY resorts to closely restricted food and drink and the administration of concentrated doses of salines in commencing or threatened peritonitis. He adopted this method recently in a case of peritonitis consequent on the presence of a malignant tumor which could not be removed. He did not use a drainage tube, but kept the patient on restricted diet and drink, applied a light bandage, and administered salines.

DR. J. PRICE thought the preparatory treatment of paramount importance in abdominal surgery. After an operation opium is simply harmful, while salines are soothing to the patient and prevent flatulent distention. Upon the slightest indication of peritonitis, use them freely and it is simply wonderful what rapid disposition is made of the thirty-seven varieties, according to Sternberg, of rhizopods and boa-constrictors. Again the drainage tube is of the greatest importance in preventing peritonitis. It usually does its work, if managed well, in the first twelve or twenty-four hours.

DR. BALDY does not think there is such a thing as dry peritonitis. The idea of administering salines is to deplete, and concentrated solutions should be used. The use of opium by the majority does not prove it to be correct.

NEWS ITEMS.

THE RESULTS ACCOMPLISHED BY THE ILLINOIS STATE BOARD OF HEALTH.—The results of the enforcement of the New Medical Practice Act in Illinois, are as follows:

The new law has required much labor, but the result has been satisfactory. Eleven suits have been pushed to successful conclusions. The most signal benefit has been in ridding the State of the itinerant nostrum-venders, with their brass-band and other accompaniments. July 1st, twenty of these shows were in the State, which tendered $1500 per month in fees for licenses, all of which were refused.

One of the evidences of the good work of the Board is the gradually falling proportion of graduates to matriculates. It is now about 31.5. In the regular schools it has fallen to 30.33.—*Sanitary News*, October 22, 1887.

A PROPOSED TEST FOR GLEDITSCHINE.—DR. CLAIBORNE, the introducer of this substance to the profession, proposes, in the *Medical Record* of November 5, 1887, to have its genuineness tested by a committee of two experts who are to make the preparation under direction, and then to pass judgment upon it. Drs. Shrady and Foster, of New York, have agreed to select the committee.

THE HEALTH OF PRISONERS.—DR. WATKINS, Inspector of the State Board of Health of Louisiana, has recently examined the prisoners in the parish prison of New Orleans. He found a number of the inmates suffering from acute dropsy of the legs, arms, face, and body, due to confinement and insufficient and unwholesome food. Each prisoner is allowed a piece of bread and a pint of tea early in the morning, and one meal consisting of soup, the beef cooked in the soup, and bread. The beef is supplied by a contractor at five cents and a half per pound, and has been repeatedly condemned by the resident surgeon.—*Science*, November 4, 1887.

HOT WATER IN SURGERY.—Possibly our readers have seen the failure in the use of hot water as a hæmostatic which DR. VARICK describes, in the *New York Medical Journal* of October 15, 1887:

An amusing instance of failure to comprehend the principles involved in the subject under discussion occurred recently in a hospital not a thousand miles from New York. A surgeon, having occasion to perform an amputation at the lower part of the leg, and after ligating the vessels, was annoyed by a somewhat free oozing which he was unable to control. A gentleman present, who had, on several occasions witnessed the hæmostatic effect of hot water in St. Francis and Jersey City hospitals, suggested its use in this case. The hot water was brought, and cold water added until it could be easily borne by the hand, then by means of a sponge it was applied for a moment several times. This proving ineffectual, the sponge, now saturated with water scarcely more than blood-warm, was held at a distance above the wound and the water allowed to trickle over the surface. It is needless to say this also failed. The verdict of the sapient oporator was that "hot water is no good."

CANNED PEAS AND COPPER.—The State Board of Health of New York has decided, after securing analyses of various samples, that the following goods are within the conditions hereinafter prescribed declared to be exempt and permitted to be sold under the provisions of the said act:

Canned peas or beans in the preparation of which copper has been used, provided that the proportion of metallic copper shall not exceed three-fourths of a grain per avoirdupois pound of peas or beans equivalent to

three grains of crystallized sulphate of copper, and that the same be plainly stated on the label.

MUTILATION OF CHILDREN FOR DEBT IN CHINA.—In the report of the Customs Inspector at Shanghai a case is reported in which a young man had lost an eye by puncture for his father's debt: the physician called in attendance was successful in saving the other eye, but the injured eye was lost. The practice is said to be common in some districts of China, and the attention of the Government has been called to it.

THE DEATH OF DR. KRAUS, editor of the *Wiener medicinische Zeitung* is announced, at the age of fifty-nine.

M. PASTEUR'S HEALTH is said to be still so feeble as to necessitate his confinement to the house.

AN INTERNATIONAL SPEECH.—At the visit of the Vienna Congress of Hygiene to Buda Pesth, the speech delivered by M. Sasvári was the most striking illustration of internationalism. He commenced in Magyar, and, after a few complimentary words to his Hungarian colleagues, assured the Austrian and German guests—himself speaking in good German—that the Hungarians recognized how much their civilization was due to the Germanic races. Then, speaking in French, he explained that Hungary admired the great French nation, which, marching in the vanguard of progress, had given to the world the highest and most generous ideas. Now, changing from French to Italian, he spoke of the glories of ancient Rome, to be revived at no distant date. Finally, concluding in fluent English, he greeted the brave children of Great Britain, who had borne the banner of civilization to the uttermost regions of the globe. Here the speech seemed ended, but, recognizing among the delegates the dignified features and high turban of the Mahomedan Burgomaster of Serajevo, he again rose, and, in the Servian-Croatian language, hailed the presence of this Oriental functionary as typifying the union of the East and the West for the furtherance of the cause of hygiene.—*Lancet*, November 5, 1887.

HEALTHY AND UNHEALTHY OCCUPATIONS.—The English Registrar-General has made a comparison between healthy and unhealthy occupations. Assuming the normal average death-rate of the community as the unit of comparison, and calling it 1000, particular occupations may be regarded as healthy or unhealthy according as the death-rates among those pursuing them fall above or below that figure. The most healthy occupation appears to be that of minister of religion, whose rate is 556. Next are gardeners and nurserymen, 599; farmers and graziers, 631; agricultural laborers, 701; schoolmasters, 719; and grocers, coal-merchants, paper, lace, and hosiery manufacturers, wheelwrights, shipbuilders, and coal-miners, with all of whom the average death-rate is under 775. The most unhealthy occupations are the trades connected with the liquor-traffic and hotel-service, with which the death-rate is 2205; following these are general laborers in London, 2020; costermongers, bankers, and street-sellers, 1879; innkeepers, etc., 1521; and brewers, 1361. After the trades concerned with alcohol, the highest rates are furnished by occupations that involve the breathing of dust—other than coal-dust—and exposure to lead-poisoning. The death-rate among butchers is also high, 1170.—*Popular Science Monthly*, October, 1887.

THE FOOD OF AMERICAN WORKMEN.—In his American Association paper on "Food of Working-men and its Relation to Work Done," PROF. L. O. OTWATER said that statistics of the dietaries of considerable numbers of Americans, mostly of the working-classes, show that their food is ample in amount, and includes large proportions of meat. Chemical examinations of the dietaries showed them to be richer in active nutritive material and potential energy than even the large quantities would imply. This is because they contain so great proportions of meat and other nitrogenous and fatty substances. Comparing the standard of diet prevailing among the working-men of Massachusetts with that of Germany, it is shown that laborers in Massachusetts average just about one-half more than the German standard requires. It thus appears that the food of the American laboring-man is much more nutritious on the average than that of his European competitors. It is also shown that he turns off much more work than the European working-man. He is better paid, better housed, better clothed, and better fed than the European. He has better opportunities for self-development, more to stimulate his ambition, and more hope of reward if his work is efficient. These factors are all connected, but the explanation of his superior capacity for work is to be found largely in his superior nourishment.—*Popular Science Monthly*, October, 1887.

THE INFLUENCE OF THE CIVIL WAR UPON THE NUMBER OF INSANE.—DR. PALMER states that "the late civil war, no doubt, has contributed toward increasing the number of insane, as well as modifying the type of mental disease. But few were rendered insane by the war that would not have broken down by other exciting causes. A large proportion of the soldiers that served in the war, and lived to return home, were reduced in mental and physical vigor. The disabilities thus acquired have influenced their children, imparting to them a delicacy of organization and susceptibility to external influences, often leading to mental disease. During the past few years several of these children have been admitted into institutions. Thus it will appear that the influence of a great and momentous event, acting upon the past, is made potent through laws of transmission to the present generation by producing mental and physical degeneracy."—*American Journal of Insanity*, October, 1887.

THE HYGIENIC VALUE OF GAS AND ELECTRICITY, AS SOURCES OF LIGHT.—The last number of the *Gesundheits Ingenieur* contains a paper by Professor Dobroslawin, of St. Petersburg, on the comparative use of gas and electricity for lighting theatres. Referring to the interesting experiments made by Professor Pettenkofer and Dr. Renk, Professor Dobroslawin, with the assistance of Drs. Kowolkowsky and Bosaroff, made, last February, during the three days' carnival at St. Petersburg, when the largest crowds fill the theatres, experiments on the temperature and ventilation of the

"Grosse" and "Alexander" theatres, lighted with gas, and the "Mariar" theatre, lighted with electricity. These three theatres gave, each, two performances daily, and each held about 1600 people. The thermometers were so placed as to be on a level with the audience. The carbonic acid was determined by the Pettenkofer method. As every theatregoer knows, the temperature falls between the acts, and it was found that the temperature in the galleries did not fall as rapidly as in the parterre. As a result, the Professor thinks that people seated in the parterre are more apt to take a cold than those in the upper parts of the house. The highest degree of temperature by electric-lighting was 25° C. (77° F.), and with gas 33° C. (91.4 F°.) The Professor does not think that the difference of air is noticeable with either kind of lighting. Finally, the Professor concludes that electricity is far superior to gas from the standpoint of health.—*Sanitary Engineer*, November 5, 1887.

THE ANALYSIS OF A "CURE FOR CANCER."—Pharmacist MOERK reports, in the *American Journal of Pharmacy* for November, 1887, his analysis of a "specific" for cancer, which has been extensively used by the inhabitants of Pennsylvania and New Jersey. It is purported to consist largely of "horse-sorrel."

The result of the analysis is that the powder contains

Moisture		0.99
Charcoal		36.82
Carbon	26.07	
Extractive	6.40	
Ash	4.35	
Arsenious oxide		62.19
		100.00

The editor states that it is probable that the powder is made by mixing two parts of arsenious acid with one part of wood charcoal, and that the deviation from this proportion is simply the result of the difference in the specific gravity of the two ingredients favoring a partial separation of the heavy arsenic. This proportion gives arsenic far in excess of that contained in the arsenical powders of Swediaur, Cosme, Dupuytren, Pluckett, and others.

DRINK LEGISLATION IN AUSTRIA.—The Austrian Government have, it is stated, introduced into the Reichsrath a strong measure for the prevention of drunkenness. The reason alleged is the alarming deterioration in the physique of young men enrolled for military service, owing to the spread of spirit drinking amongst the humbler classes. It will be interesting to see if the provisions of this measure are accepted. They would be difficult to carry out in this country, where freedom even to get drunk is still jealously guarded. Spirit shops are to be closed at five on Saturday afternoon, and to remain so till five on Monday morning. Ordinary storekeepers are not to be allowed to sell spirits, the sale of which is to be restricted to public houses, confectioners, and specially licensed stores. Dealers serving spirits to intoxicated persons are to be liable to arrest or fine. No debt for spirits consumed on the premises can be recovered by law if the amount exceeds the value of five quarts. The magistrates are empowered to forbid retailers to serve spirits to habitual drunkards for any period up to twelve months. Similar legislation already exists in Galicia. The proposal now is to extend it to all Austria. The preamble of the measure sets forth that it is a direct outcome of the Hygienic Congress.—*Lancet*, November 5, 1887.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT U.S. ARMY, FROM NOVEMBER 8 TO NOVEMBER 14, 1887.

GREENLEAF, CHARLES R., *Major and Surgeon.*—Par. 8, *S. O. 248, A. G. O.*, October 25, 1887, directing Surgeon Greenleaf to visit the recruiting depots and rendezvous at certain places, is amended to include Davenport, Iowa; Quincy, Ill.; and Evansville, Ind.—*S. O. 257, A. G. O.*, November 4, 1887.

LORING, LEONARD Y., *Captain and Assistant Surgeon.*—Ordered for duty at Fort Mojave, Arizona Territory, upon the expiration of his present sick leave of absence.—*S. O. 258, A. G. O.*, November 5, 1887.

BYRNE, CHARLES B., *Captain and Assistant Surgeon.*—Relieved from temporary duty at Fort McHenry, Maryland, and will return to his proper station, Washington Barracks, District of Columbia.—*S. O. 242, Division of the Atlantic*, November 11, 1887.

PERLEY, HARRY O., *Captain and Assistant Surgeon.*—Now on duty at Fort Wayne, Michigan, ordered for temporary duty with troops stationed at Highwood, near Chicago, Illinois.—*S. O. 258, A. G. O.*, November 5, 1887.

IVES, F. J., *First Lieutenant and Assistant Surgeon.*—Granted leave of absence for one month, to take effect on or about the 15th instant.—*S. O. 113, Department of the Platte*, November 5, 1887.

WALKER, F. V., *First Lieutenant and Assistant Surgeon.*—Relieved from duty at Post of San Antonio, and assigned to duty at Fort Ringgold, Texas.—*Department of Texas*, November 8, 1887.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY FOR THE WEEK ENDING NOVEMBER 12, 1887.

HEYL, T. C., *Surgeon.*—Detached from the "Marion," proceed home, and wait orders.

CRAIG, T. C., *Passed Assistant Surgeon.*—Detached from the "Marion," proceed home, and wait orders.

WELLS, H. M., *Medical Inspector.*—Detached from the "Trenton," proceed home, and wait orders.

PECK, GEORGE, *Medical Director.*—Ordered to Washington, D. C., as a member of the Examining Board.

AYRES, JOSEPH G., *Surgeon.*—Ordered to the "Galena," to relieve Surgeon F. L. DuBois.

DUBOIS, F. L., *Surgeon.*—Detached from the "Galena," proceed home, and wait orders.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE, FOR THE TWO WEEKS ENDING NOVEMBER 12, 1887.

WYMAN, WALTER, *Surgeon.*—To proceed to Louisville, Ky., and Memphis, Tenn., as inspector, November 2, 1887.

URQUHART, F. W., *Passed Assistant Surgeon.*—Relieved from duty at Cape Charles Quarantine; ordered to Norfolk, Virginia, November 6, 1887.

MAGRUDER, G. M., *Assistant Surgeon.*—When relieved, to rejoin station at Chicago, Illinois, November 3, 1887.

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.*

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

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